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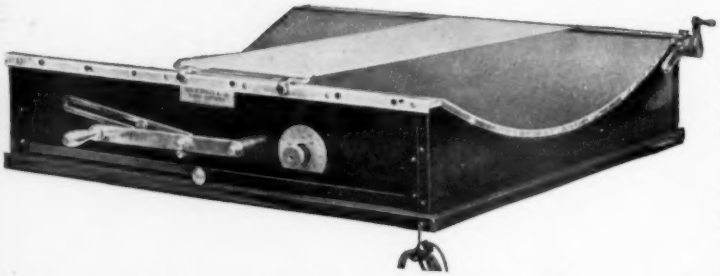

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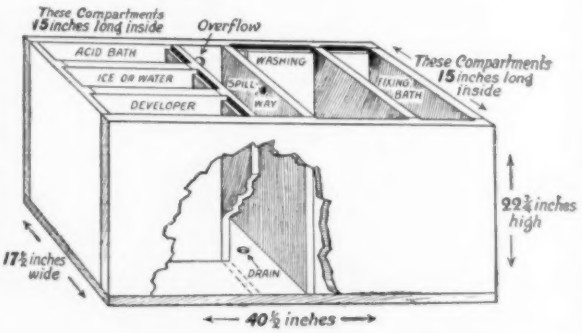

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THE AMERICAN JOURNAL OF ROENTGENOLOGY AND RADIUM THERAPY

VOL. X JUNE, 1923 No. 6

CARCINOMA OF THE SMALL BOWEL*

BY MILTON M. PORTIS, M.D., AND SIDNEY A. PORTIS, M.D.

CHICAGO, ILLINOIS

CARCINOMA of the small bowel is a comparatively rare lesion. Ewing states that it comprises 3 per cent of intestinal carcinoma. Of the 41,858 necropsies at the Vienna General Hospital, 3,585 were cases of carcinoma. Of these, 343 were in the bowel, and only 11 of these were in the small bowel. Hinz found, among 584 cases of carcinoma of the intestinal tract, that only 18 were located in the small bowel. Johnson, in a recent paper, reported 2 cases of carcinoma in the jejunum and one in the ileum. Bevan reported 5 cases of cancer of the jejunum. Cancer of the duodenum is reported by Lichty in 6 cases, McGuire, 4 cases, Herman, 2 cases, Jefferson, Deaver and Head each, 1 case. Judd reported 24 cases, 5 of which were in the duodenum, 11 in the jejunum and 6 in the ileum, and 2 multiple cases. About 70 per cent of the cancers of the duodenum are located at the ampulla.

These tumors of the small intestine, according to Ewing, are of three distinct forms:

1. Part of a local or general intestinal polyposis. This is rather rare in the small intestine, being more frequent in the colon. This type probably explains the occurrence of multiple carcinoma with several strictures produced.

2. Multiple or single benign embryonal carcinoid tumors, which are usually found in the ileum and jejunum in the form of single or multiple firm opaque nodules as large as a pea or bean. They lie in the mucosa or submucosa. In structure they form three groups:

(*a*) Pancreatic island tissue.
(*b*) Heterotropic intestinal mucosa.
(*c*) Brunner's glands.

3. Localized adenocarcinomas arise under various conditions, from single intestinal polyps, and tend to maintain an adenocarcinomatous structure. The majority of the intestinal carcinomas are of this type. When ulceration is delayed, the tumors may reach sufficient size to obstruct the lumen. Ulceration and stricture more commonly result. Metastases are present in about one-third of the cases, and involve the mesentery, liver, lungs and peritoneum.

The clinical picture is variable, depending on the type and age of the carcinoma. In the early cases, before obstruction, there may be no subjective symptoms, and the patient may not be aware of the presence of the tumor until obstructive symptoms appear. Although hemorrhage is uncommon, yet, even in the earliest stage, erosion occurs, and occult blood is found in the stools. This is of greatest importance in an early diagnosis.

As is common in carcinoma in any part of the body, loss of weight and anemia take place. Asthenia and a vague abdominal discomfort are complained of. Later, pain is felt, and this increases as obstruction begins. It may be colicky and paroxysmal, especially when there is much tympanites. A progressively developing constipation is complained of, especially

* Read at the Twenty-third Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Los Angeles, Calif., Sept. 12–16, 1922.

with definite stenosis at the tumor site. At times, constipation does not appear at all, but diarrhea is present. Rarely, constipation alternates with diarrhea. Very soon, stomach symptoms appear. Belching and nausea, anorexia and thirst are complained of, and later vomiting begins. The vomiting is frequently repeated as often as 6 to 8 times daily, and the vomitus is usually of a greenish color. In a well-developed case it may have a brownish color and a fecal odor. The typical picture of intestinal obstruction appears with the closure of the stricture.

The triad of symptoms which are of diagnostic value is a stubborn constipation, with or without alternating diarrhea, severe attacks of colicky pain, and vomiting.

Objective symptoms are not found early, but with developing stenosis there is tympanites, visible peristalsis, and a tumor may be palpated. The tumor is usually small and firm. It is movable and changeable in location. With marked vomiting there may be pronounced dehydration and signs of severe intoxication.

Laboratory studies of the gastric contents do not show evidence of disease. The stools rarely show gross blood, but occult blood is usually found, and is the most reliable of the early findings. The *x*-ray is of great help in the diagnosis. A normal stomach and duodenal bulb help to rule them out as a cause for the occult blood. The duodenum is usually dilated and likewise, the small bowel proximal to the stenosing tumor. Gas and fluid are found in the distended bowel. The amount of dilatation found will naturally vary with the duration of the tumor, and the amount of stenosis produced by it. In the cases in the literature, an early diagnosis was not made, and only at operation for acute obstruction was the tumor found. This is too late for a cure. A routine examination of all stools for occult blood will be of decided help in discovering these lesions early enough for operative aid. In the differential diagnosis, tuberculosis must be ruled out carefully. Actinomycosis at times gives a similar picture and rarely, syphilis.

We wish to report a case referred by Dr. Plummer in 1917. A woman of fifty-two complained of nausea and vomiting and rapid loss of weight. She had considerable abdominal distress. There was marked tenderness in the upper abdomen, but no mass could be felt. The stomach test was normal and the stool showed occult blood constantly. The *x*-ray examination made by Dr. Jenkinson revealed a normal stomach and duodenal bulb, but there was a delay in the emptying time. The duodenum was dilated, and likewise, the jejunum proximal to a definite constriction in it. The right lower lobe of the lung showed several small, round, solid areas, which suggested metastases.

Because of the uncontrollable vomiting, Dr. Plummer operated and found a ring carcinoma of the jejunum near the duodenum. The mesenteric glands were definitely involved. A gastrojejunostomy was done. Unfortunately, the plates in this case have been misplaced.

A second case was referred to us by Dr. Christofferson, and it is through his kindness that we are presenting it to you. A male, thirty-four years old, complained of epigastric distress, vomiting, loss of weight, weakness, night sweats and tinnitus. For the past six weeks he had had abdominal distress with swelling on the left side. The swelling was intermittent and accompanied by cramp-like pains, and visible peristalsis. Vomiting occurred at a later stage, and he brought up food eaten twenty-four hours or more before. At times the vomitus had a fecal odor, and was green to yellow in color. The trouble came on at intervals. At first the attacks occurred once a week, but later, as often as twice a week. Between attacks he felt fairly well. An enema brought no relief. In the previous six weeks he had lost 20 lbs., and felt very weak. He had had no night sweats. Constipation began several weeks before and was now very stubborn. Bowels would not move without an enema.

Physical signs of the heart and lungs were normal. The abdomen was distended, but no peristalsis was visible. No mass could be felt. There was tenderness on the left side, just above the level of the umbilicus. The stomach test was normal, except for delay in emptying. The stool showed a

marked reaction for occult blood. His Wassermann test was negative, and there was a secondary anemia. The Von Pirquet test was slightly positive. The urine showed a high indicanuria.

tion showed a definite constriction. Reversed peristalsis was seen in the dilated portion proximal to the constriction. Operation was advised for a probable tumor of the jejunum. Dr. Christofferson found

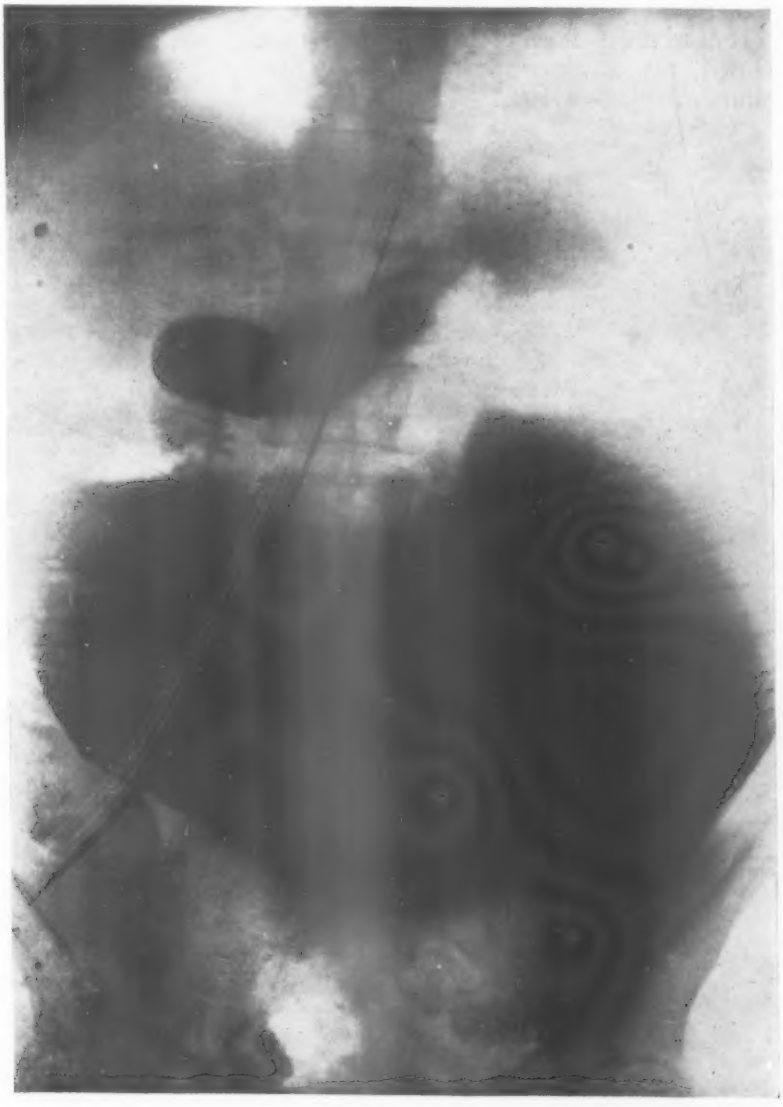

FIG. 1.

The *x*-ray showed normal chest findings. The stomach was in a transverse position, but was normal in its findings. The duodenum was dilated. The small bowel near the duodenum was filled, and remained so, giving the appearance of a stomach with air space above a fluid level. (See Fig. 1.) The bowel at the point distal to the dilata-

an annular growth in the jejunum near the ileum. A resection and a lateral anastomosis were carried out.

A specimen of the excised tumor was shown to Dr. Schultz, who reported as follows:

"*Gross.* Resected segment of small intestine 5 cm. long. At its middle there is

a rather sharp constriction 2 cm. in external diameter, in which the external surface for half the circumference is formed by a slightly raised, firm, pale area 1 cm. wide in the longitudinal axis of the gut. Above the constriction the gut is 4 cm. in diameter; below it, 2.5 cm. In the region of the constriction a pale, rather dense, well-defined, mushroom mass projects into the lumen; this is 2 cm. wide in the long axis of the gut. (See Fig. 2.)

FIG. 2.

"*Microscopic.* Both above and below, the transition of normal mucosa to tumor is sudden and sharp. The tumor is composed of stroma of variable density, in which are embedded atypical glandular alveoli. The latter vary in shape and in size; many are branched, and in many the epithelium is infolded. The epithelium in general is of the intestinal cylindrical type, but in many alveoli it is more atypical and polyhedral. Mitoses are numerous. The entire intestinal wall is penetrated by the tumor mass, which has replaced muscle and the other constituents of the wall." (See Fig. 3.)

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DISCUSSION

DR. MOORE. I enjoyed Dr. Portis' presentation very much. Certainly these cases of malignancy of the small bowel are sufficiently rare to be of unusual interest, and yet they do occur as a definite thing, and therefore we must, as roentgenologists, be on the lookout for them. Personally, I have seen two cases of malignancy of the small bowel, with location of growth high in the duodenum. One was an adenocarcinoma. There was a six-hour retention in the stomach and marked dilatation of the duodenum proximal to the growth.

FIG. 3.

The second case was a growth situated in the duodenum, but close to the jejunal angle, and caused no obstruction in the stomach. However, it caused a marked retention of the six-hour meal in the small bowel proximal to the growth, and there was extreme dilatation.

Both of these cases were operated on. The first case was an adenocarcinoma of metastatic origin. The second case was operated on and left the year following.

The thing that impressed me was the extreme degree of prostration in those two cases. It seemed to me that the prostration was considerably in excess of the complaint of the patients as to their length of symptoms and their discomfort and blood picture, the anemia not being particularly marked in either case.

Dr. Jaches. While I appreciate the compliment the chairman has paid me, I really do not know why he asked me to discuss this paper, except, perhaps, that he wanted to keep me indoors so that I could sit here and enjoy the presentation of Dr. Portis.

Personally, I have seen no such case, to the best of my recollection. The only lesion of the smaller bowel which caused dilatation and almost presented a picture similar to the one shown by Portis, was a type that he excludes in his paper—one that occurs in the cecal area.

I am glad, however, to have heard this, because, although the condition is very rare, we will nevertheless be on the lookout for it; and we can never tell when a condition which occurs, say once in ten thousand cases, will strike us very hard.

I simply want to thank Dr. Portis for having given me an opportunity to listen to his paper.

Dr. LeWald. The following 4 cases occurred in the service at St. Luke's Hospital: One was in the second portion of the duodenum and caused obstructive symptoms; that was an adenocarcinoma. It was simply explored and the diagnosis confirmed. Another case was operated on and the tumor in the jejunum resected. I do not have complete notes of the final outcome.

Another case, also in the jejunum, caused partial obstructive symptoms. The fourth case was one that had been operated on by another surgeon, who had simply performed an intestinal anastomosis to the abdominal wall, and the question came up as to where the connection had been made. I injected sodium bromide solution 20 per cent through the fistula and made a diagnosis of jejunal fistula. If you are in doubt about what to use in examining a sinus, you may use sodium bromide solution with perfect satisfaction. It acts as a cathartic if it reaches the bowel and is promptly eliminated. It is the same solution we use in obtaining a pyelogram.

Dr. Portis (closing discussion). I am very glad to hear Dr. LeWald report that he has found such cases. As Dr. Jaches has said, any one of you might unexpectedly be confronted with a case of this type. They are very easy to diagnose.

Two cases which I saw I recognized very quickly, largely after excluding the stomach by laboratory and *x*-ray methods. The *x*-ray evidence is clean cut. When you find occult blood in the stool, and you have a picture of the individual I described, you can fairly definitely assume that you are dealing with a possible malignancy. At least it is surgical, and if the surgeon should not find malignancy, there still is definite indication for operation, and you are wise in advising it.

ROENTGEN GASTROINTESTINAL STUDIES OF PATIENTS WITH CHRONIC DEFORMING ARTHRITIS*

BY R. G. TAYLOR, M.D.

Hospital of The Good Samaritan

LOS ANGELES, CALIFORNIA

THIS subject was approached originally from the surgical side, and, so far as we are personally concerned, has been studied largely from that viewpoint, due to the fact that most of the work that we have done has been done for surgeons.

Starting with the proposition that the cause of chronic deforming arthritis is an intestinal infection, probably, more specifically, a perverted bacterial flora in the intestines with secondary joint infections, these roentgen studies have been pursued. It is recognized that the theory of the gastrointestinal origin of this type of arthritis is not universally accepted. Nevertheless, holding this theory, we have made the gastrointestinal studies with it as a basis.

These studies have been made by giving a single meal, examining the upper tract, and then at six, nine, eleven, and twenty-four hours making observations, chiefly with the screen, but with some films for record purposes. Thereafter observations are made each twenty-four hours until the final disposition of the meal is determined.

FIG. 1. Case 8701. Twenty-four hours p.c. Mobile colon, except tip of cecum which is fixed by appendix, adherent, high up inside of ilium. Pouching downward of movable colon forms pocket, which fails to empty in seventy-two hours. Findings confirmed at operation.

FIG. 2. Case 8469. Seventy-two hours p.c. Deformity of cecum caused by retrocecal adherent appendix making pocket in cecum which does not empty in seventy-two hours. Findings confirmed at operation.

An enema is given if the distal colon is not fairly well visualized, or if there is anything noted with the meal suggesting pathology.

In a general way, the things we have observed have been: First, the majority of these patients have a certain type of colon characterized typically by its abnormal length and abnormal mobility. Second, practically all the cases that have become chronic show so-called surgical lesions in the right colon, or in the ileum, or in both. Third, these lesions consist of adventitious

* Read at the Twenty-third Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Los Angeles, Calif., Sept. 12–16, 1922. Discussion of this paper and the others in the same symposium will appear in a later number of the JOURNAL.

bands similar to Jackson's membrane, involving the colon, usually above the ileocecal valve, and involving occasionally the ileum, producing the so-called Lane kink and other less typical fixations, or chronic appendicitis, or the results of an acute inflammatory process in the appendix with adhesions and fixation of the cecum and appendix to surrounding structures, notably the ileum and right colon itself (the appendix is frequently retrocecal), or post-operative adhesions following removal of the appendix.

We believe the adventitious bands are usually congenital or are developmental processes, the result of the abnormal mobility of the right colon. Some of them may be the result of local inflammatory processes in this region, chiefly from the appendix; but there are apparently two fairly distinct types of cases: those in which the appendix and the results of its inflammation are the chief factor, and those in which the adventitious bands usually showing a little higher in the colon are the chief factor.

The presence of these bands and adhesions results in a crippling of the intestine, which is evidenced roentgeno-

FIG. 3. Case 8034. Twenty-four hours p.c. Cecum descending into pelvis with band making torsion in this position above ileocecal valve. Colon distal to band rotated, showing appendix on the right of cecum.

FIG. 4. Case 8034. (Same case as in Figure 3.) Forty-eight hours p.c. Cecum raised out of pelvis. Tension on band relaxed relieving rotation. Appendix in normal position on the left and normal outline of colon. Findings confirmed at operation.

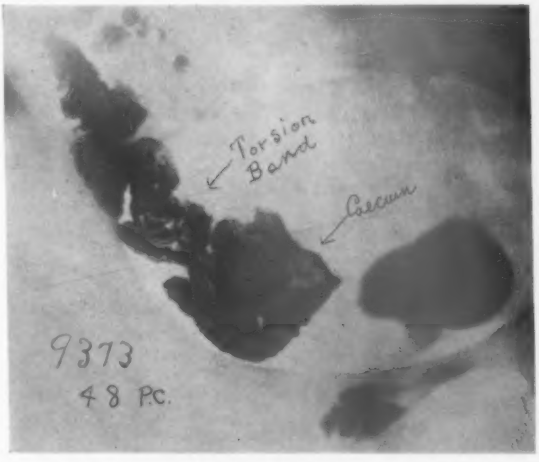

FIG. 5. Case 9373. Forty-eight hours p.c. Rotation of cecum by band when descended into pelvis. Twist of bowel shown by partially filled rugae. Findings confirmed at operation.

logically by certain deformities in the barium shadow and evidence of torsion or twisting or kinking of the gut with resulting constriction; also in a fairly large percentage of cases by a definite local stasis proximal to the lesion.

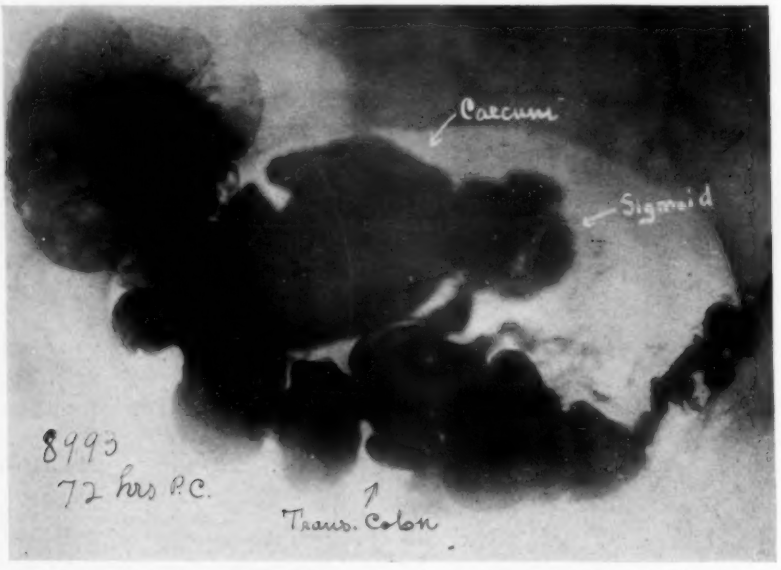

FIG. 6. Case 8993. Seventy-two hours p.c. Anomalous position of cecum at center line in the pelvis; very movable, low hepatic flexure and transverse colon. Torsion in the ascending colon about its middle with practically no emptying at seventy-two hours. This is probably congenital over rotation without fixation at the flexure. Findings confirmed at operation.

FIG. 7. Case 8571. Seventy-two hours p.c. Almost entire retention of meal in right colon and hepatic flexure. Constricting band in transverse colon, just distal to flexure. Findings confirmed at operation.

In connection with the theory of intestinal infection it seems reasonable to believe that a local stasis distinctly favors infection and perversion of the normal flora. This has been well demonstrated in other portions and organs of the body. The interest of the roentgenologist consists in the demonstration of the lesions which probably are responsible in most cases for the crippling and stasis.

Granted that there is an infection, and that there is a crippled portion of the intestine, it would seem reasonable to assume that the crippling of the intestine, especially if it produced local stasis, is a factor in the continuance of the infection, and certainly must be taken into consideration when an attempt is made to bring about a normal condition. We therefore believe that these lesions must be done away with before marked permanent improvement in the local condition can be expected. A certain percentage of these cases that show some crippling but no actual stasis would seem to offer a chance for pure medical treatment; but the others are distinctly surgical.

We believe that the roentgenologist can, in a general way, segregate these two classes from his findings, and also that he can, with a reasonable amount of experience and observation, in the surgical cases, tell the surgeon the type, location, and kind of lesion to be expected, and give him some idea as to the degree of normal function being performed by the bowel.

A check-up of the roentgen findings with the operative findings in between 30 and 40 cases shows a very gratifying percentage of correct diagnoses, the chief place of error being in the ileum, pathology being ordinarily found there when indicated by the roentgenologist, but also occasionally being found when not shown by him; error in reference to the colon and cecum being practically negligible.

The results of the treatment of these cases are not to be included in this paper except to say that with proper roentgen selection a reasonable proportion of the so-called non-surgical cases have shown satisfactory and gratifying improvement. Of the surgical cases, about 50 per cent of those whose operation is remote enough to determine the final result, have shown practically complete arrest of the disease. The other 50 per cent are graded from "improved" to a few that have shown no improvement. The results have been gratifying enough in this practically hopeless type of disease to make us feel that we are warranted in further pursuing this subject and developing its diagnosis and treatment.

The purpose of this paper is to call again the attention of roentgenologists to the importance of roentgen studies in these cases, to the value of the findings, and to the very distinct and easily demonstrated types of lesions that are almost invariably found in the gastrointestinal tracts of these patients; also to impress on the internists who are interested in treating this disease with the intestinal infection theory of causation as a basis, the futility of attempting to get permanent results in the cases which show frank surgical lesions.

It is to be noted that this type of long mobile colon, with adventitious bands, or with bands and adhesions due to past or present appendix inflammation, has been found in many patients who do not show arthritis. However, a careful review of these non-arthritic patients shows them invariably to be subject to symptoms that are most easily explained as being the result of an intestinal infection. As to why these patients do not have arthritis and others do, is a matter that is still unsettled and under discussion. We hope some day in the near future to report the studies on these patients.

With regard to the routine examination of patients in the manner above outlined, we have tried numerous short-cuts, all of which appear to have been lacking in some essential, and have been discarded. We have come to the conclusion that the additional and more accurate information gained well repays the roentgenologist for the extra work involved.

A COMPARISON OF CLINICAL AND ROENTGENOLOGICAL OBSERVATIONS IN PULMONARY TUBERCULOSIS

BY FRANK S. BISSELL, M.D.

Roentgenologist to the Minneapolis Clinical Association

MINNEAPOLIS, MINNESOTA

INTRODUCTION

THE conviction that close cooperation between the lung specialist and the roentgenologist are essential in the diagnosis of pulmonary disease has led the writer to enlist the assistance of Dr. W. J. Marcley in this study. Out of 265 cases examined for the U.S.P.H.S. during part of 1919 and 1920, 114 cases, in which a satisfactory examination had been completed by both of us, were selected and the result of the study tabulated.

Total Number of Cases in Series	114
R. Positive—C. Positive	59
R. Positive—C. Negative	0
R. Negative—C. Negative	15
R. Negative—C. Positive	4
R. Indeterminate	36
Analysis of Indeterminate Group	
Total	36
Called probably tuberculosis	26
Clinically positive	23
Clinically negative	3
Called probably not tuberculosis	10
Clinically positive	6
Clinically negative	4

It is perhaps unfortunate, from a scientific point of view, that any diagnosis of tuberculosis in its earliest stages may justly be questioned on the ground that there is no single infallible test, uninfluenced by the judgment of the examiner, by which the correctness of such a diagnosis may be proved. Nevertheless, careful observation for a sufficient length of time, skilful elicitation and conservative interpretation of physical signs, study of pulse and temperature curve and the determination of sensitiveness to tuberculin administered subcutaneously, will produce a mass of evidence which should be acceptable in any medical court.

The cases reported herein were checked by clinical studies as outlined above, and by independent roentgen-ray observations. A similar study with very similar results was made in collaboration with Dr. E. T. F. Richards of St. Paul, in 1913, and published in the AMERICAN JOURNAL OF ROENTGENOLOGY.

ROENTGEN TECHNIQUE

It seems necessary to emphasize repeatedly two important considerations, in the hope that they may eventually find a place in all criticism of the roentgen diagnosis of pulmonary disease:

1. That the lung roentgenograms, from which safe deductions may be drawn, must be of the best quality, prepared according to a standard technique.

2. That much intensive study and large experience are quite as essential to intelligent interpretation of roentgenograms as they are to other special types of medical procedure.

Important factors in technique are:

(*a*) Position of patient (vertical, breast against plate, scapulae thrown outward).

(*b*) Tube distance and shift (accurately correlated).

(*c*) Exposure time (not exceeding one-tenth second).

(*d*) Tube (fine focus).

(*e*) Accurate time development.

THE ADVANTAGES OF COOPERATION

It is indeed disheartening in this day of medical advancement, to observe so frequently men without sufficient experience to appreciate the pitfalls, attempting to interpret lung roentgenograms which would be wholly unreadable to the most experienced. The writer would not discourage the laudable efforts of those interested in lung diseases to familiarize themselves with *x*-ray diagnosis, but would merely emphasize the danger of relying too much upon their own roentgenological deductions until they have given the subject as much or more study than they have found it necessary to devote to other diagnostic procedures.

The roentgenologist with his greater experience in the interpretation of lung stereograms could be of much greater assistance to the clinician if he were more in the latter's confidence. There is a well-marked disposition on the part of most clinicians to withhold (consciously or inadvertently) clinical data in their possession from the roentgenologist. From a practical standpoint this is ill-advised, since it may lead the roentgenologist into avoidable error, and this in turn, may mislead the clinician himself. The roentgenologist should be treated as any other consultant, and supplied with all information available. He is then in a position to confirm, refute, or supplement the clinical findings.

When the history of the early development of this work has been written, the name of Kennon Dunham will stand pre-eminent as that of the one who has done most to place the interpretation of lung stereograms upon a pathologico-anatomical basis. In our opinion, however, too much reliance has been placed upon the so-called "fan" or "cone." While this is, doubtless, a sign of great value when present, many correct diagnoses of early pulmonary tuberculosis can and must be made without its aid. We stress this point because many workers in this field have been led by Dunham's emphatic statements to search their stereoscopic plates only for the cone, and not finding it have discredited the roentgen method by failing to make the correct diagnosis. Dunham's latest publication[1] is an admirable piece of work, in which there is everything to commend and little to criticize. It is one of the most noteworthy contributions to the medical literature of our day. It is significant that in this article, he does not stress the value of the "cone" as he has done in previous papers.

ROENTGENOLOGICAL OBSERVATIONS

A careful review of a large series of known tuberculous cases of many different types and in various stages of development has failed to show a single instance in which there were not abnormal lung markings in the periphery of the upper lung field beyond the distribution of the terminal bronchi. It would seem, therefore, that for practical purposes at least, the rule might be laid down that a negative diagnosis is indicated when the lung parenchyma within this part of the lung field is free from all evidence of infiltration. While it is true that tuberculosis may chiefly involve the lower lung fields, this study seems to indicate that there are always some signs of the disease within the area described. These tuberculous lesions take the form of fine tubercle-like shadows, sometimes in the form of small conglomorate patches, and sometimes not. If the disease is active, the lesions usually have a cloudy appearance, and there is no perfectly normal lung area between them and the hilus. If, on the other hand, the disease is arrested, healed, latent, or of the concealed type, these patches are less cloudy in appearance and the lung area around them appears relatively normal. There are a few other pathological conditions which produce changes similar to these in the upper lung fields. Jarvis[2, 3] has proved their occurrence in marblecutter's disease. These exceptions to the rule make the positive diagnosis somewhat less reliable than the negative, but experience in recognizing the differences in the character of these various shadow formations minimizes the danger of error. The literature upon this subject contains frequent references to peribronchial infiltration, the inference being that these are somewhat characteristic of tuberculosis. Our observations lead us to conclude that peribronchial infiltration is much more characteristic of other infections or bronchial irritants and that they should receive little consideration in the diagnosis of tuberculosis. A certain type of hilus thickening frequently associated with calcareous deposits has some contributory value, since it indicates a past tuberculous infection which may have become reactivated. Evidence of fibrosis may be accepted as pointing to a tuberculous process when it occurs in the upper lung fields or when there are other signs more or less suggestive of that disease.

CAVITATION

Tuberculous cavities may usually be recognized by dense annular shadows, with-

in which normal lung markings cannot be seen. Patches in the lung with absolutely dense centers, indicating a caseating process, are usually present with such cavitations. The cavity of bronchiectasis is differentiated by its thinner wall and more cylindrical contour. Pulmonary abscess, theoretically, cannot be differentiated from a filled cavity, but the latter is a rare finding in the study of roentgenograms. The small circumscribed pneumothorax is difficult to differentiate from a cavity situated near the pleura, but this again is a rather uncommon location for a tuberculous cavity.

DETERMINATION OF CLINICAL ACTIVITY

Our statistical observations seem to indicate that the roentgen method is of great value in determining the degree of activity of those cases in which tuberculous lesions are present. In those cases diagnosed inactive the subsequent history or course of the disease agreed as a rule with the *x*-ray observations. Cases diagnosed as probably active showed a somewhat larger percentage of error, the evidence of activity probably being due to mixed infection. It is here that clinical evidence, as shown by temperature curve and case history, is of the greatest value. Our roentgen reports show an increasing number of cases classified as tuberculous, but with the question of activity indeterminate. This is because we have come to believe that the question of activity can usually be determined by clinical evidence, and that the province of the roentgen-ray examination is rather that of determining the site and extent of tuberculous lesions, presence of complications or the complete absence of clinical tuberculosis.

It should be borne in mind that the more characteristic signs of early tuberculosis are probably caused by lymphatic engorgement and local congestion, and hence will disappear as the process becomes inactive.

A diagnosis of clinical tuberculosis, based upon the presence of old tuberculous lesions without evidence of recent activity, tends to discredit the roentgen method.

ROENTGENOLOGICAL PROGNOSIS

In a previous paper, "Concerning a roentgen conception of pulmonary tuberculosis,"[4] an attempt was made so to classify various types of tuberculosis from a roentgen standpoint that roentgen reports might be of greater value in establishing a correct prognosis. Briefly stated, the various factors upon which prognosis could be based were:

1. The type of infiltration (either discrete or conglomerate).
2. The extent of lung involvement.
3. The presence of old healed lesions in other parts of the lung.
4. The amount of fibrosis.
5. The presence of cavities and areas of caseous pneumonia.

Thus in type A, so-called concealed tuberculosis, the prognosis is favorable as to life, but less favorable as to complete recovery of normal health.

Type B is more frank tuberculosis with typical clinical history, with marked fibrosis and no cavitation. In this type, prognosis is favorable under proper management.

Type C represents those cases with an early tendency to cavitation, in which there are massive conglomerate shadows in the roentgenogram indicating the presence of caseating pneumonias. This is the most unfavorable type from the standpoint of prognosis.

Type D, healed tuberculosis, shows the characteristic scars, broken linear markings, the pleuritic cap, the calcification, etc. It is thus seen that stereo-roentgenograms of the lungs, when properly studied, are of great value in arriving at a prognosis.

While the roentgenologist cannot, with conservatism, venture a prognosis in a given case, he may aid the consultant by emphasizing those factors in the report which are a direct indication of the probable course of the disease. Thus a simple diagnosis of fibroid tuberculosis is insufficient, unless qualified by a full description of all observations including cavitation, caseation, disseminated and discrete lesions, and their distribution.

Such a classification as this one serves the further purpose of clearly pointing the way

TYPE A

Roentgenological	Clinical	Pathological
1. Infiltration, peripheral 2. Tendency to unilateral involvement 3. Limited areas of involvement 4. Areas of previous involvement indicated by increased fibrosis 5. Progression from one area to another, shown by observations over period of years 6. Absence of cavitation 7. Absence of massive or conglomerate infiltration	1. Poor nutrition. Exhaustion with slight exertion 2. Occasional slight temperature upon exertion 3. No physical signs 4. Reaction to subcutaneous tuberculinfocal, general, or both 5. Favorable prognosis	

TYPE B

Roentgenological	Clinical	Pathological
1. Fine soft shadows of infiltration both peripheral and peribronchial 2. Direct extension to hilus 3. Characteristic distribution in 1st and 2nd interspaces (preferably right) 4. Dunham fan more common 5. No cavitation 6. Marked fibrosis 7. Progression to general involvement of one or more lobes 8. Localized density over apex (one or both)	1. Temperature curve 2. Intermittent physical signs 3. Loss of weight with intermittent gains 4. Prompt tuberculin reaction 5. Sputum frequently negative 6. Favorable influence of climate 7. Infinitely chronic	1. Infiltration 2. Fibrosis 3. Pleuritic thickening especially over apices 4. Noncaseating 5. Absence of amyloid changes 6. Absence of cavities

TYPE C

Roentgenological	Clinical	Pathological
1. Early signs similar to Type B 2. Massive conglomerate shadows make early appearance 3. Marked tendency to cavitation	1. Physical signs typical and early 2. Hemoptysis often occurs 3. Bacilli in sputum early and constant 4. Progression unremittent 5. Unfavorable response to climate changes or sanitarium treatment 6. Frequent presence of focal infection of tonsils or teeth (especially the former)	1. Infiltration 2. Caseation 3. Cavitation 4. Amyloid 5. Fibrosis

TYPE D

Roentgenological	Clinical	Pathological
1. Absence of characteristic parenchymal shadows and of Dunham fan 2. Areas or nodules of great density (calcification) 3. Broken lung fields (fibrosis) 4. Evidence of pleuritic "cap" over one or both apices with localized extension of fibrosis-like shadows into lung field	1. No clinical signs	1. Calcification 2. Fibrosis 3. Thickened pleura

to complete cooperation and understanding between the roentgenologist and the clinician.

A little knowledge of the clinical history will not lead the roentgenologist, if he is trustworthy and experienced, into the error of reading observations into his plates. On the other hand, there are so many times when he may properly say, "Here is the pathology—what is the history?"

Then, too, such knowledge always serves to arouse a keen interest in the case, thus sharpening his powers of observation. The art of diagnosis has become so complex with the development of new methods and minor specialties that the attainment of the best results demands the free interchange of thought and the closest coordination of ideas.

THE ROENTGEN REPORT

Since the communication between the roentgenologist and the clinician is usually a written one, a concise terminology based upon a clear conception of the disease is an essential requirement. While observations may be described in terms of variable densities, conclusions must be a translation of these observations into pathological terms. With such a pathological conception established, an opinion as to the actual disease present is a logical sequence. Such an opinion may be positive in character when observations have been sufficiently characteristic to justify the opinion that a tuberculous lesion is present. The opinion may be qualified by a statement as to the probable activity or inactivity of the disease, and a suggestion as to prognosis.

A report may be negative in character, and this should mean that there is no evidence in the roentgenogram of pathological change in any sense characteristic or even suggestive of tuberculosis.

The report may be indeterminate, which should mean that there are changes present which are neither sufficiently characteristic to justify the conclusion that they are due to tuberculosis, nor clearly enough of negative value to warrant a negative diagnosis.

An indeterminate roentgen diagnosis may very properly be used with considerable frequency, but should not be employed as a subterfuge, and should always be fully explained in the body of the report.

A re-study of the plates in the series of indeterminate cases has convinced the writer that most of them may be placed with accuracy in either the positive or the negative group. In fact, it was found that the observations described in the body of the report usually gave a direct indication of the group to which the case belonged.

Tubercle bacilli were later found in the sputa of three of this group, but a re-examination of the plates showed abundant and characteristic evidence of tuberculosis. This shows that the error was due to hasty observation and carelessly drawn conclusions on the examiner's part, not to a fault in the method.

THE NEGATIVE REPORT

The negative report has for some reason not been given the high place in the ensemble of diagnostic data which it deserves. Such a report, when made by a competent roentgenologist, under favorable conditions, may well be accepted as conclusive, even though all other signs seem to indicate pulmonary tuberculosis. While no effort should be spared to exclude the ever-dangerous element of human error, it is doubtful whether a diagnosis of pulmonary tuberculosis is ever justified in the face of repeated negative roentgen-ray findings.

A careful review of our series of plates in conjunction with the subsequent histories of patients has so far failed to reveal any instance where the stereoscopic plates were entirely negative, and the case later proved to have been pulmonary tuberculosis.

CASE REPORT

Male. Entered St. Mary's hospital for study on Feb. 5, 1921. Had been in service since Jan., 1918. Had influenza in Sept., 1918. Quite ill for ten days and coughed up clots of blood. Has had slight cough since that time. No loss of weight. No fever at any time, except first three weeks of Jan., 1921, when he noted temperature of 99°F. or over. Tires readily. Occasional night-sweats in Jan. On Jan. 27th, "without warning coughed up about a cupful of clear

blood" followed by expectoration of small amounts. Following day, two hemorrhages, one large amount, one smaller. Remained in bed until coming to the hospital.

Physical examination by Dr. Alexander Josewich. Chest. Patient well nourished and developed. Head and neck negative. Heart normal. Lungs: Upper left, moderate dullness, bronchovesicular breathing and typical indeterminate rales.

Diagnosis. Tuberculosis, pulmonary, chronic, active. Upper lobe left.

ROENTGEN REPORT FEB. 9, 1921

Observations. Slight diffuse relative density over the left lung field. No evidence of active or recent pulmonary tuberculosis. Dense band across left lung field. Right lung negative.

Conclusions. Possible pleuritic thickening (adhesion or thickened interlobar pleura). Negative as to tuberculosis.

Following this examination, the patient was sent to a tuberculosis sanitarium in the Southwest, thence to Washington for further study, and later to California, where he was in a tuberculosis sanitarium for several months. Although many sputum examinations were made, tubercle bacilli were never found. On Mch. 15, 1922, the patient had another small hemorrhage and reported for another examination on Mch. 16th. At this time, physical signs were found in the upper left lung field more marked than on previous examination. Stereoscopic plates were made and read as follows:

"There is no roentgen-ray evidence to support a diagnosis of tuberculosis. Whatever changes are present in the lung field are at the base, chiefly on the left side, and there certainly is nothing upon which one can base a diagnosis of tuberculosis, from an *x*-ray standpoint."

Death occurred in an anginal attack on Mch. 18, 1922, and the report of the autopsy made by Dr. Carl A. Danielson is as follows:

"The right pleura had a number of fine adhesions from top to bottom, which could be broken up by the hand, thus freeing the lung. The left pleural cavity contained one large band of adhesions about the middle part of the upper lobe and the lung was freed from it with difficulty. This part of the adhesions apparently constricted this portion of the lung so that it was more solid. That is to say, it would not crepitate as readily as the rest of the lung. The apices of both lungs were free. The left lung, near the hilus, was in a state of chronic passive congestion. There was no evidence microscopically of tuberculosis, either active or healed, though I am not certain but that Dr. Bell may find something in the specimen sent down.

"There was a beginning atheroma of the aorta, and the coronary arteries were sclerosed. The cause of death was thrombosis of the left coronary artery.

* * * * * * *

"Today I have a letter from Dr. Bell with the report that there is almost a complete closure of the left coronary artery. The lungs show a little subpleural scar tissue. There is no evidence whatever of tuberculosis."

INDETERMINATE REPORT

About 20 per cent of our cases are classified as indeterminate on the ground that there are atypical changes in the lung fields which render a negative diagnosis inadvisable and a positive diagnosis dangerous. Certain cases of chronic pleurisy in which there is, or has been, an exudate, and in which the entire lung field appears abnormally dense on account of pulmonary infiltration, fibrosis or induration of the lung tissues, must be classified as indeterminate, since it is impossible with these changes present to exclude tuberculosis.

Then there are the postinfluenzal infiltrations, streptococcus bronchopneumonias and their sequelae, such as bronchiectasis, which must occasionally be classified as indeterminate with respect to tuberculosis.

POSITIVE ROENTGEN REPORT

In rendering a positive diagnosis of pulmonary tuberculosis, based solely upon roentgen evidence, one is beset with danger on every hand. A pulmonary infarct associated with bilateral infiltration may not even be considered, and its close resemblance to an active tuberculous process makes the slip easy and the fall hard.

There are the passive congestion of chronic heart disease, the so-called dust inhalation diseases and the streptococcus infections mentioned in the previous paragraph which must be carefully borne in mind, and if possible, differentiated; however, if reasonable care is taken in preparing and interpreting lung roentgenograms, and if the interpretation is founded upon broad roentgen experience and pathological knowledge, the positive roentgen diagnosis of pulmonary tuberculosis will be correct in a large percentage of cases.

This paper should be received as supplementary to certain others published by the writer during the past ten years. There is no attempt to present any new material, but merely to emphasize certain conclusions which time and experience have strengthened:

1. That cooperation between roentgenologist and clinician are highly important in the diagnosis of chronic lung disease.

2. That the relative values of various diagnostic procedures employed for the recognition of early tuberculosis are as follows:

History and symptomatology
Roentgen study
Sensitization to tuberculin
Physical signs
Laboratory tests

The roentgen examination is placed second, because, while lesions may be demonstrated in the stereo plates of practically all cases, their exact significance is determined by the case history and the symptoms.

Again, the symptoms per se have great diagnostic value since experience has shown that the early symptomatology of tuberculosis is always suggestive.

Dr. Marcley regards the tuberculin test, when properly applied in small doses of tuberculin, as of great value. A high sensitization to tuberculin, conjoined with positive roentgen-ray findings, however slight, justifies a positive diagnosis. Our study indicates that the mere presence of positive signs in the upper lung field should not have too much weight in the diagnosis of pulmonary tuberculosis. While we have placed the laboratory examination last in the list, the work of Larson[5] at the University of Minnesota may elevate laboratory procedures to the head of the list. This, however, will require time for development and proof. The finding of tubercle bacilli in the sputum is a rare occurrence in early tuberculosis, and even when present does not absolutely prove the existence of pulmonary tuberculosis.

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PULMONIC AND CARDIAC CHANGES FOLLOWING INOCULATION WITH FOREIGN PROTEIN

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I. ADULTS

THOSE of us who were doing intensive investigation of early pulmonary changes in the type of infection prevalent during the great pandemic, were struck by the prevalence of a high percentage of these cases showing parenchymal changes without symptoms and the almost universal involvement of the hilums in all cases, even when there were no subsequent pulmonary sequelae. Further study of these cases showed that mottling of mediastinal and bronchial root tissues still remained several months after the subsidence of all clinical signs. Lord, in his chapter on circulatory disturbances, states that "inflammatory hyperemia, too slight in degree to be termed pneumonia, may be seen in the course of influenza and typhoid fever." Localized and sometimes general pulmonary congestion occurs, as is well known, after injection of tuberculin. Pulmonary mottling is quite commonly seen after inhalation of certain substances, as in hay fever. This is not so much by reason of their mechanical trauma as by their anaphylactic import. In the routine *x*-ray examination of influenza cases, attention was drawn to a radiogram in which general mottling was of a different type from the usual case. Two changes were sufficiently great to warrant further investigation, with the following interesting history:

The patient had received the usual dose of 0.5 c.c. of triple typhoid vaccine (army), and within thirty-six hours he was prostrated, and developed headache, malaise, back pains, short irritable cough and temperature (not a rare experience). On physical examination there was increased dulness and bronchovesicular breathing, and a few indeterminate rales were found from the 4th to the 2nd interspace on the left side. Clinically, because of the epidemic, a diagnosis was made of influenza with possibly a bronchopneumonic process. The clinical findings and the history of inoculation made an absolute diagnosis seem undesirable. As was the rule at this time during the epidemic, the patient was immediately examined radiologically. The plates showed a localized area of increased density from the 4th to the 2nd rib on the left side, and as there were none of the characteristic diaphragmatic and cardiac reactions to which Honeij has called attention, we felt that a definite diagnosis of influenza was unjustifiable.

Within thirty-six hours the temperature dropped to normal and the chest cleared up completely. Subsequent radiograms showed that the process was much improved and mottling was almost absent. Following this finding, the next 8 cases inoculated with typhoid vaccine were examined radiologically before inoculation and forty-eight hours later. Seven of these cases were supposedly "normal" chests; the eighth was one of extensive and non-active tuberculosis. The following findings were obtained :

Not one of them failed to reveal increased density, either pulmonary or hilum. In 6, this was general in character; in 6, the apices were involved; in 2, the middle thirds of both lungs were affected; in 1, the bases were involved. In 6, the upper aspect of the hilum was distinctly enlarged and mottled. In 7, the middle portion of the hilum showed these changes, and in 7 the lower portion of the hilum. In 5, the heart showed slight pulmonic dilatation; in 4, it showed right-sided dilatation. In 6, there was distinct increase in the peribronchial markings. In 1 case there was an increase in the size and density of the mediastinum; and in 1, glandular enlargement of the left middle hilum.

These incidental findings are analogous to those observed by investigators in cases of laboratory animals whose death was due to anaphylactic phenomena.

INFANTS AND YOUNG CHILDREN. INFLUENZA GROUP

No.	Increased Density	Parenchymal Site	Heart Enlargement	Hilum	Remarks
1	General + +	Left perihilum Perihilum +++	+ −	Right hilum + +	
2	General + −	Middle-right Left perihilum	+ +	Both hilums + −	
3	General + −	Left perihilum	+ −	Left and right ?	Peribronchial type of increased density
4	General + +	Left perihilum++ Right middle +	+ −	+ −	
5	General + −	Right apex	+ +	Both	Increased density limited to hilums and left apex
6	0	0	+ −	0	Only slight mottling of hilum present
7	0	0	0	0	No change observed
8	Slight General	.....	+	Right Left perihilums	Heart only showed changes

DIPHTHERIA TOXIN ANTITOXIN GROUP

No.	Increased Density	Lobes	Heart Enlargement	Hilum	Remarks
9	Peribronchial + −	Right apex	0	Right	
10	General +	All lobes	+++	Left ++ Right +	Questionable cardiac lesion
11	General	Upper right	+	Both	Left-sided cardiac enlargement
12	Local	Left upper	(Slight) +	Right	
13	General	Right base	Left sided ++	Both	
14	?	Left lower	Right sided (?)	Right	
15	Local	Right lower Right middle	++	Both	
16	Local	Perihilum	+	Both	Upper mediastinal enlargement
17	Local	0	+ −	Both (rt.) +	
18	Local	Right upper	+ −	Right	
19	Local	Right upper	+ −	Both	Changes of upper mediastinum
20	0	0	Slight enlargement	+ −	
21	Local	Right middle Left upper	++	Both	
22	Local	Right upper	+	Left	
23	Local	Right lower	+ −	Right	
24	Local	Right upper	0	Right	

II. INFANTS AND CHILDREN

Further investigation of these phenomena, employing the same roentgenological technique, was carried out upon a group of infants numbering 24, of whom 16 had been prophylactically vaccinated with diphtheria toxin antitoxin. In this series the findings were essentially the same as in our adult group when typhoid vaccine was used.

As shown by the accompanying table, mottling was local or general and practically universal. In one case only the heart showed some reaction by dilatation.

The remaining 8 children were examined after prophylactic treatment with influenza vaccine. Here, too, our findings confirmed

our conclusions drawn from the work with typhoid vaccine and diphtheria toxin antitoxin.

In cases of food idiosyncrasy, as demonstrated by positive skin and constitutional reactions, with or without pulmonary anaphylactic signs, we have been able to demonstrate this intrapulmonary change as a concomitant anaphylactic phenomenon.

SUMMARY

Here we have an investigation covering a group of 33 individuals varying in age, the larger percentage of whom, after injection of foreign protein presented definite pulmonary changes and, in a smaller percentage, cardiac changes.

THE X-RAY IN NEUROLOGICAL DIAGNOSIS: ITS SHORTCOMINGS AND POSSIBILITIES*

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THE difference in density of the tissues involved markedly limits the use of the *x*-ray as a diagnostic aid in the varied pathology of the central nervous system. Previous to the intraventricular or intraspinal injection of air by Dandy in 1918, the neurologist did not expect the radiologist to do more than eliminate or demonstrate the presence of bone changes in the skull or vertebræ, or calcareous degenerations within the skull proper.

A careful review of the literature of reported cases of brain tumor during the last five years shows that 3 per cent of the new growths within the skull contain ample calcareous degeneration which is radiologically demonstrable. There is another 2 per cent of lesions of the central nervous system interpretable from an *x*-ray angle. Such lesions consist of the osteophytes of the inner plate of the calvarium, located mainly in the frontal area; growths of the orbit, either of an osseous nature or of sufficient size so that pressure will thin the bony wall to a noticeable degree; tumors within the ethmoid, sphenoid or frontal sinuses which through pressure effect destroy their bony boundaries; and dural tumors which through pressure thin the calvarium or seek and secure circulatory connections with the diploë, producing diploic varices. Pathology of the hypophysis, according to the literature of the last three years, when carefully tabulated, using the Journal reports as a basis, furnishes another 3 per cent which correlatively can be diagnosed by the *x*-ray. The summary of these three types of neurological lesions gives radiology an 8 per cent direct diagnostic aid ratio. Ventriculography and spinal intra-arachnoid injections of air, through their indirect aid in density shadings, have brought the demonstration of and the location of intracranial and intraspinal lesions through the *x*-ray as a diagnostic method up to a fair 52 per cent, correlatively speaking.

Neurological diagnosis previous to the epoch-making use of air by Dandy was as follows: Given a story of persistent, intractable headaches, alone or accompanied by projectile vomiting, choked discs, labyrinthine symptoms, manifested by the existence of a non-normal nystagmus or vertigo, changed reflexes, psychic derangements, muscular incoordination, an *x*-ray demonstration as to the existence or absence of skull or vertebral structure change or presence of calcareous deposit, coupled with proper serological findings, and the neurologist would decide as to the existence of a lesion and its probable location.

Many scientific factors have been correlatively blended, to accomplish a semblance of accuracy in neurological diagnosis.

Epoch-making neurological diagnostic aids have arisen during the last decade and a half. Barany's observation in 1905 of a definite type and directed nystagmus accompanied by distinct vertigo when

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suppurative ears were douched with cold or hot water, led naturally to the present caloric test. Flourens, in 1825, made an excision of portions of the labyrinths of animals and noted that as a result this caused movements of the eyes and definite disturbance of equilibration. Purkinje, at the same period, noted that human beings when turned about, developed a nystagmus and vertigo. Meniere, Goltz, DeCyon, Von Stein of Moscow and Hogyes, a Hungarian, made definite studies along similar lines, the latter two contributing a valuable summary of the anatomy and physiology of the inner ear. All these observations were weighed, as well as Von Graef's pointing test in ocular palsy, by Barany, and the chair test of nystagmus,—the head in varied positions to utilize the anatomy of the semi-circular canals,—and the past-pointing vertigo syndromes of Barany came into existence, a veritable summary of several scientists' observations. The value of the Barany past-pointing nystagmus and vertigo manifestations in basilar and cerebellar-pontine fossa lesions has produced and is still productive of heated controversies between otologists as to its place in diagnosing tumors of the brain. A large part of this controversy between those interested in neurology is due to inadequate or lack of open-minded study of the Barany syndrome.

Lesions of a developmental or accidental nature, about the pons or medulla, or along the individual or associative tract, pursued by the 8th nerve from nucleus to termination, are diagnosed through nystagmus and equilibration defects. Equilibration is an intimate association of three senses, namely, sight, muscle, and kinetic static. The utricle, sacculi and the three semicircular canals of the internal ear have been accepted as a separate and distinct organ, thus making two for the inner ear, the cochlea, the organ of hearing, the saccule, utricle and the three semicircular canals the organ of equilibration. Variation from the normal, secondary to the use of the caloric or Barany tests, gives the neurologist a neuro-otological aid that must be weighed in interpreting a possible pathology in the silent areas of the brain.

Acuity of vision may be unimpaired in early brain tumor, but the color sense is frequently decidedly encroached upon at an early period, particularly in chiasmic area lesions. Pituitary pathology promptly intrudes on the color sense. Internal hydrocephalus, by third ventricle enlargement compresses the chiasm, with the resultant effect of a rapid edema of the optic nerve disc. Communicating hydrocephalus, particularly the type that is produced as a result of intrapendicular or chiasmic cistern occlusions, intrudes on the chiasm with a resulting optic nerve change which is expressed in the choked disc of varied degree.

Berthold's studies in 1848 of the transplantation of a cock's testes; Addison's monograph in 1855 attributing a definite clinical syndrome to a diseased destructive process of the suprarenal capsule; Bernard's investigation of the hepatic function and demonstration of an internal secretion (glycogen as well as the external secretion, bile) led easily into a line of organotherapeutic interest, popularized by Brown-Sequard. Knowledge of disorders of the thyroid, a frequent etiological factor in lesions of the central nervous system, was given the light of day. This included the addition by Liegeois, sixty odd years ago, of the pituitary to the ductless gland groups and endocrinology. Cushing's epoch-making studies of the pituitary led to the recognition of the separate functions of the anterior and posterior portions of the gland. The careful summary of the objective and subjective symptoms, among which color-sense defects assume a prominent place, shows that the pituitary, not only in a direct but in an indirect sense, assumes a prominent position in the lesion of the central nervous system.

These rapidly scanned points bring to the elucidation of central nervous system lesions the aid, then, of the histologist, the physiologist, the ophthalmologist and the representative of a new branch in neurological diagnosis, namely the neuro-ophthalmologist. To the radiologist has been entrusted the chief place in demonstrating the presence or absence of lesions of the hypophysis. By utilizing the radiographic formula of Leri, which is (1)

enlargement of the anteroposterior or of the vertical diameter of the sella turcica; (2) enlargement of the frontal sinuses; (3) irregularity of the thickness of the calvarium; (4) exaggeration of the external occipital protuberance: by blending with the above Paccini syndromes of variation of angles, the pathology of the pituitary is radiologically clarified.

Paccini uses a wax pencil and marks the skiagraph of the skull first at the nasion, second at the turcicon, a hypothetical point placed in the sella space where the imaginary long and short diameter lines intersect at right angles, and third at the akoustion, a point in the uppermost ovoid shadow cast by the external auditory meatus, connecting nasion to turcicon, turcicon to akoustion. He then notes the degree of angulation which such lines assume. The age, sex and race of the patient must be considered, along with the variation of angulation. It is insisted that one will thus be able to recognize the finer changes of pituitary disease, more especially the changes involving secretion, or better, physiological function. This remains as yet a debatable point. There is perhaps a marked value, yet there is needed a decided degree of careful investigation and innumerable observations to make these assertions absolutely tenable. By blending Leri and Paccini points, interpretation of the endocrine neurological trouble, acromegalia, as well as hypofunction of the pituitary, can be radiologically demonstrated.

Dandy's work for you, as well as for us, your neurological confreres, has been, speaking in a diagnostic as well as localizing sense, epoch-making.

"Jacobeaus used the therapeutic insufflation of air as far back as 1909, and ten years later tried out the diagnostic possibilities on a case of spinal tumor. In an article which appears in the *Acta Medica Scandinavica* for Dec. 5, 1921, lv, 6, he reports in great detail 3 cases including the proceedings. Another Scandinavian, Josofson, reports a single personal case, and apparently these 4 make up our knowledge of the successful cases."*

* Editorial. Diagnostic Insufflation of Air into the Spinal Canal. *Med. Rec.*, Mar. 11, 1922, ci, 422–423.

Jacobeaus, while preceding Dandy in the use of air in subarachnoid spaces, failed to publish his investigations in the use of air in neurological diagnostic work. Pneumography of brain and cord is distinctly Dandy's device, safely bulwarked in the fortress of independent study and priority of publication. Other agents beside air should be considered. Hydrogen gas has a markedly increasing capacity of density contrast over air and is not irritating to the delicate structures of the cord or brain, but is not so accessible, hence will never be popularized.

The technique, proven safe in carrying out ventriculography and spinal intra-arachnoid injection of air, has a few seemingly simple yet essential points. To neglect these points is so dangerous that it seems fitting to mention them in some detail. The first is a careful selection of equipment. With an ordinary 20 c.c. record syringe which has a two-way connecting tip such as is used in aspirating fluid from a chest, a couple of carefully selected platinum spinal needles, a reliable mercurial spinal manometer, and a small syringe for anesthetizing the skin and subcutaneous tissues, one is equipped to carry out the subarachnoid injection of air.

The intraventricular method needs, beside the above equipment, a knife, needles, artery forceps and a Doyen burr. Carefully prepare the scalp, then mark the same, either along Chiene's, Kocher's or LeFort's method of cranio-cerebral topography, locating the midpoint of the sagittal line. Then, if one accepts Kocher's plan, separately mark an anterior 80° point. Next, at a point 2 cm. either to the right or the left of this, mark the place for trephining. All this work can be done under local anesthesia. This point to the right or left of the anterior 80° spot of the sagittal point is fairly accurate for tapping the anterior horn of the lateral ventricles. The next important feature is the taking of the pressure of the fluid, whether from spine or ventricle, before any escapes, and measuring accurately the quantity removed. Atmospheric air has been proven clean. Its ordinary room temperature is satisfactory for injection. The force that should be used should be based upon the mano-

metric readings. One should not insert a greater quantity of air than the amount of cerebral spinal fluid removed. The double spinal needle technique of Adolph Bingel is unnecessary. It is a dual mechanical insult to the dura. Drain all the spinal fluid that will escape. Have the head and shoulder of the patient 20 per cent higher than the lumbar region. Inject the air slowly. Disconnect the syringe now and then from the spinal needle. Wait a couple of moments, or until the ascending air has displaced the descending cerebral spinal fluid, the latter being carefully caught and measured. Repeat the injections of air as often as it is needed to secure a positive cessation of fluid flow.

The ventricular injection does not call for the use of a needle in each ventricle, although if careful equal exchange of air for fluid is maintained there is no danger, as has been proven, from removal of all the cerebrospinal fluid in the ventricles. Where the technique of double needles is carried out, one is used for fluid escape, the other for air entrance. The ventricles are tapped at two slightly separated periods. Fluid is permitted to escape after its pressure has been manometrically read, the head being turned so that the first tapping is of the superior ventricle. Then the lower ventricle is tapped, and the outflow of fluid from the lower needle sucks in air from the upper, ample to fill ventricles, and at an ordinary atmospheric room pressure.

This is the method used in hydrocephalic states of young children, whose fontanelles have not yet closed. It is through the outer angle of these, of course, that the tapping has been done. In the ventricular technique in adults one should not attempt tapping both ventricles unless one has developed a dry tap, a condition resulting from a tumor occluding the foramen of Monro or a compression of the ventricle one is attempting to drain, by a tumor in the adjoining tissues. Such a result should induce one to trephine and to attempt to tap the opposite ventricle.

Ventriculography beautifully demonstrates the internal, as well as the communicating type of hydrocephalus. Occlusions of the aqueduct of Sylvius or of the foramen of Magendie and Luschka, have differentiating *x*-ray points. Obstruction of the foramen of Luschka and Magendie gives a wide distention of all four ventricles and a practically dry spine. Stoppage of the aqueduct of Sylvius produces a wide dilatation of the lateral and third ventricle and an almost empty spinal arachnoid.

The occlusion of the cisterns, particularly the chiasmatica or interpeduncular cistern, is not alone accompanied by a high spinal fluid pressure, but when the phenolsulphonephthalein test of Heur and Dandy is given, the urine content in two hours may show as low as 8 to 12 per cent of phthalein instead of the normal 35 to 45 per cent. The physiological fact that four-fifths of the cerebrospinal fluid is absorbed from the cerebellar subarachnoid space, and one-fifth from the spinal neurolymphatic spaces has been well proven. The phthalein test but utilizes this fact.

Internal hydrocephalus needs distinctly a ventricular tapping. In communicating hydrocephalus the spinal injection of air is ample. The clinical story of an adult, when properly studied, leads to a fair pre-tapping interpretation as to type. In young children with open fontanelles the ventricular route is preferable. There is no part of cerebral roentgenological interpretation that demands a greater knowledge of the normal than ventriculography. The location, size and contour of the ventricles must be firmly fixed in the mind of the man interpreting the plate. Compression of one of the lateral ventricles by contiguous growth may be either in its body or in one of the horns. One should remember that the ventricle next to the plate in a ventriculograph of an adult or child is not demonstrated. It is the superior ventricle that shadows. Fluid is below, air above. The latter skiagraphs, the former does not. Hence the need of a double lateral skiagraph and both an anteroposterior and a posteroanterior plate, using a Dixon's 13° pillow.

Lesions in the cerebellopontine fossa occlude the 4th and frequently the 3rd ventricle, particularly if the growth is of an acoustic-nerve or brain-stem origin.

Lesions of the island of Reil have a tendency to occlude the body of the lateral ventricles. Each of these pathological central nervous system changes has an accompanying feature, interpretable from other special men's line of work. Lesions of the brain stem and cerebellopontine fossa have equilibration involvement, manifesting itself in nystagmus, vertigo and past-pointing changes. Growths in the internal capsule or corpora radiata areas have eye, labyrinthine and pyramidal tract symptoms. Each area of the brain, silent or known, in a sensory, motor or mental sense, has some individual accompanying feature. Ventriculography will demonstrate many brain-stem, cerebellum, and cerebellopontine fossa lesions, but interpretations of such from a distinct *x*-ray angle should not be undertaken. A correlative study of every angle of investigation of the case should be made; and then, and not until then, should the radiological expert hazard an opinion. Without correlative study such an opinion is but a presumptuous hazard, unscientific and uncalled-for.

Spinal intra-arachnoid air injections demonstrate the location of extra and subdural lesions quite accurately by the subjective symptoms of pain as well as the radiological demonstration of difference in density in the intra-arachnoid space. Intramedullary cord tumors hardly lend themselves to air interpretations. Obliterations of cerebellar sulci by cortical lesions are readily interpretable to one who has a working idea of the normal. Failure to demonstrate a certain section of the cortex when air has been used for interpretation is decidedly indicative of a lesion at that point. To induce the best in this new radiological method a series of normals should be collected and published. Without the use of air within the ventricles or intra-arachnoid space of brain and cord, the radiologist cannot aid further than summarized in the opening page of this paper, in diagnosing the central nervous system lesions.

Aneurisms of the internal carotid, psammomatous changes in pineal, pituitary or Pacchionian structures, brain-stem growths, calcareous changes in gliomatous cysts, arteriosclerotic changes in cortex and fissure areas, calcareous deposits, single and multiple, and even a complete calcareous degeneration of the entire brain, have been reported in medical literature in the last five years and have been interpreted by means of *x*-rays.

There is a great opportunity in undifferentiated stupor cases for roentgenological effort. The separation of the traumatic from the toxic or nephritic type of case can be markedly aided by anteroposterior, posteroanterior and laterally made skiagraphs of the skull. Each is a wet brain. Each is a stupor state due to compression of base and cortex. Tapping of the spine is therapeutic in the two latter and diagnostic by the finding of blood in the spinal fluid in the former.

Careful *x*-ray technique will frequently demonstrate extra- and subdural blood-clots. The presence of such differentiates compression from concussion. Spinal-fluid findings of blood confirm the suspicious shadows. On account of its many morbid characteristics the traumatic type demands early diagnosis. It is true that an early narrowing of the pupil, a later wide dilatation of same, or a trigeminal or seventh-nerve objective paretic symptom upon the side of injury, is frequently observed. These should serve as interpretable points; but often they are absent or have conflicting developments that preclude their serving in differential diagnosis.

The basilar fracture case, as Cushing and Sharp have amply proven, should be drained. The temporal decompression technique of Cushing has been utilized by Sharp, as well as by its originator, in hundreds of cases. A rubber-dam drainage of the incised temporal dura, with simultaneous removal of the clot, if easily accessible, and silverclip occlusion of vessel, if it is readily to be located, is the logical recognized technique today. Surgical intervention should follow an early diagnosis. Delay means increased surgical risk and the development of undesirable morbidity results.

Since the publication, years ago, by Little, the English orthopedist, of his observations of spastic paraplegia in children (now denominated Little's syndrome), it has been recognized that such

was but the sequence of intracranial hemorrhage occurring at birth. Until recently too little attention has been paid to the immediate symptoms of the newborn, the later effects of which are now so well known.

The newborn that shows any of the following, either as single or group symptoms, should have a skiagraph made of its skull in the classical anteroposterior lateral method: poor suckling; irregular breathing; a slit-like, contracted pupil, later dilated; ecchymotic spots about the temples or eyelids; or dilated veins of the scalp. Children prematurely born, forcibly expelled by a multipara, are liable to a distortion of their delicate skulls with the result of rupturing the vessels of the dura, tearing of the tentorium-cerebelli, or developing petechial hemorrhages in the various nuclei neighboring the ventricles or the brain-stem. Children born of primipara approaching the end of their child-bearing period, instrumentally delivered or torsion-handled, are, of course, peculiarly liable to such intracranial trauma. There is another type manifesting two or more of the above-listed symptoms. These have had a perfectly normal birth, but despite this give evidence or suggestion of intracranial pathology. Such is the child with a navel or intestinal hemorrhage symptom. This type has a slow coagulating blood-finding and a prolonged bleeding period. The need of early diagnosis in these varied types of afflicted newborn is apparent. If they survive the first few days and are improperly treated, they are apt to develop into simple-minded, hemianopic, spastic or epileptic children. The radiologist can aid in an early diagnosis.

Fissured fracture is the type occurring in the newborn patient in a trauma of the bone. Fissured fractures follow in a radiating way from the centers of ossification. The parietal eminence is a frequent base of such fissured fractures. Intra- and extra-dural clots should be demonstrated by a soft technique. The treatment of this pathology of the newborn is location of the clot by radiology, followed by surgical handling of the case. Cushing calls the newborn a spinal type of animal. Literature reports show that the risk is light in comparison to the results in the untreated case. If the child can be saved from the subsequent well-known morbid developments, surgical chances should be taken. This is a field well worthy your skill and needs the utmost finesse in methods to differentiate the type, preceding surgical efforts.

The spinal types of pathology differentiated for the neurologist by radiology can be briefly listed under the head of the varied forms of spinal spondylitis and new growths, each developing bone changes. Syphilis, whether in the newborn (the congenital type) or in the adult (acquired tertiary type), leaves its stamp upon cortex of bone and mainly upon the shaft of the long bones. Charcot's joint is the one outstanding exception to this pathological rule. The toxic irritation of the periosteum in this disease leaves the tooth-edged shadow of Pacini, or the chalk line of increased compact structure of Post, radiologically observable in the adult. Bela Alexander, noting the epiphyseal absorption points, and the independent similar findings noted by Shipley, Pearson, Weech and Greene, in the newborn, lends to this type of neurological pathology a bone interpretation that differentiates syphilis from other types of spondylitic lesions.

Infectious arthritis is also an osteoclastic cell stimulant. Carcinoma and tuberculous and sarcomatous lesions are osteoporotic developers. Bone destruction is radiologically demonstrable, when either of these pathological states is the etiological explanation of a compression myelitis. Typhoid spondylitis may or may not be bone-destructive. It is a question of the period of the vertebral presence of the disease. Early skiagraphs of typhoid bone lesions demonstrate bone overgrowths; late skiagraphs, a bone-destructive process. Correlative study of case history and physical findings should be used in differentiating the varied forms of spondylitis. Carcinoma is practically always metastatic. Sarcoma is nearly always a primary lesion. Tubercular spines may be primary, but nearly always have a concomitant pulmonary lesion or are accompanied by an involvement of some other joint. Syphilis

has a story of infection and serological proofs of its presence. Typhoid has a history of a recent attack of protracted intestinal trouble.

With spine neurological differentiations in mind, the radiologist should always insist upon a case history. Many central nervous system troubles have distant points of manifestation. Morvan's type of syringomyelia, with its changes in the first phalanx of the digits, the cupula-domed diaphragm, manifestation of phrenic nerve injury or involvement in cord lesions, is one of these. The clinician and the radiologist must be constant consultants. Insistent painful backs in the lower dorsal or upper lumbar region, varying in degree, frequently spell Kuemmel's disease, an unrecognized compression fracture of the body of the vertebræ with an over-bone growth about the transverse process. Sacralized fifth lumbar vertebræ, radiologically located, frequently explain the protracted type of sciatica.

There is, then, for the radiologist a wonderful field in neurological diagnosis. Shadow summation in skull skiagraphy needs an intimate knowledge of the base formation and the suture lines of the skull for proper interpretation. To us who are interested in central nervous system lesions, you are very essential consultants. Your efforts and the labors of the serologists, the neuro-otologists and the ophthalmologists must correlate with the clinical story and our physico-neuro findings, to make the diagnosis of central nervous system lesions approach accuracy.

In conclusion I have but these perhaps presumptuous suggestions to make: Remember that you are consultants, and important ones, too. Remember that the clinical history of the case is as essential to you, if you wish to be accurate in all branches of your labors, as it is in neurological interpretation. Demand of your confreres the fullest of cooperation in communicating to you the complete findings of others before you render an opinion in an interpretative sense. This is due you; moreover, the patient is entitled to cooperative conclusions. Others are hazardous to them as well as to you.

Ventriculography as well as intra-arachnoid injections of air will remain, I hope, the labor of the neurological surgeon or become a new field of roentgenological effort only after a safe technique has been acquired by the latter medical man. Trephining the skull under a local anesthesia, using a small Doyen burr, is not difficult, it is true; tapping the lumbar arachnoid space does not demand an excess of surgical skill; yet each needs an anatomical knowledge and an aseptic technique which, in my opinion, makes it, for the present, the province of the neurological surgeon, and not the radiologist.

The demonstration of the unoccluded lateral ventricle as well as the filling of the third and fourth ventricle with air needs another special line of study. The recognition of the normal as to contour, size and relationship of these structures makes of this new indirect roentgenological agent a very interesting bit of study. The use of air in cortical and ventricular shadowgraphy is but another indication that soon your specialty will be further divided into therapy and skiagraphic divisions. There will be a new and separate branch entitled, in all probability, "Interpretology." The latter will be based upon a special course which medical schools should now have and be prepared to give to the man making a specialty of roentgenological work. It would seem logical that roentgenology should be subdivided in its teaching courses into roentgenological pathology, roentgenological histology and roentgenological physiological branches, besides the ones at present pursued. Thus will be developed diagnostic roentgenological experts. This is not a criticism; it is merely a suggestion.

Radiological central nervous system interpretation, in my opinion, should be builded upon a correlatively examined clinical story and the use of other diagnostic aids. Roentgenological reports, unfortunately, are only too frequently rendered without a proper study of the history of the case. There is a definite and distinct *x*-ray pathology which is markedly separable from macroscopic and microscopic tissue changes. The surgeon, wishing to be right, rarely voices a macroscopical

interpretation without a knowledge of the clinical story of the case; the microscopist frequently errs who attempts a lens interpretation summary without considering the patient's history; and yet your average medical colleague anticipates from you a positive diagnosis without the aid of a collated history or an understanding of the other man's findings. This is unfair to you and unjust to the patient. Demand of your confreres an accompanying clinical story of the case which he requests you to diagnose radiologically. Roentgenology has a high diagnostic place to maintain. You are consultants in the fullest sense of the word. Other consultants of special types demand a pitiless publicity concerning every feature of the case before even starting their investigation. Do likewise.

DISCUSSION

Dr. Stewart. I wish to mention the routine examination of unconscious patients brought into the hospital. We have found that our statistics with reference to the presence of fracture have greatly increased since we routinely sent them to the *x*-ray department for examination. It is very important to examine the entire skull. You may otherwise overlook something that would be very valuable to the surgeon.

Dr. Schultz and his associates have quite persistently found that *x*-ray findings are of value as indicative of injury to the brain substance on the opposite side. They claim that the strength of the blow is sufficient to force fluid around and injure the brain substance at that point. One of the great difficulties in ventrilography is the sustaining of the needle through the opening of the skull while the air is injected; and Dr. King of New York has devised a very ingenious thing: a screw which screws into the opening of the skull. On that his needle rests on ball-bearing arrangements, so that it is always supported.

Dr. Crouse (closing discussion). In conclusion I want to say that the selection of your needle for tapping the brain is very essential. It is a great deal of trouble to maintain the needle in the ventricle. I am glad to hear of Dr. King's technique. We are anticipating great things from the radiologist and we sincerely trust that in another year you will say "By this method and by that method, and by this technique and that technique we can safely aid you in 85 per cent of the cases." We think it is coming and we are looking to you to do it.

TRANSLATIONS AND ABSTRACTS

Stevens, Rollin H. Advances in radiation therapy of deep-seated tumors. *J. Mich. M. Soc.*, Mar., 1923, xxi, 124.

This is a review of our knowledge of physics covering the principles of deep therapy. The author reviews in detail the work which has been done by the various German clinics and refers to some of the theories of the biological effect of *x*-rays. A part of the article is devoted to an interesting discussion of the question of surgery in cancer. The author doubts the expediency of radical operation in most cancers for the following reasons:

1. The removal of all the reproductive elements of cancer by the most radical surgical operation is probably impossible, in the great majority of cases. The history of the surgical treatment of cancer substantiates this statement.

2. There is much evidence that the connective tissues about a cancer are important factors in the cure of the disease, and their help can be greatly increased by stimulation with radiation. These tissues, therefore, should not be removed surgically, or destroyed by any other means.

3. Radiation does destroy cancer cells, directly or indirectly. Whether directly or not, it probably stimulates the connective tissues about the growth so that they proliferate, crowd out the cancer cells, and possibly secrete immunizing, biological substances against cancer.

4. Metastases are usually more sensitive to radiation than the primary growth. (Exceptions to this are cases of metastases in the neck.) Therefore, if surgery is required the author believes it should be limited to the primary growth; and the sterilization, so to speak, of the cancer field should be left for radiation to accomplish.

Pre-irradiation is better if given four to five weeks before surgery. Thus is the field made safer for operation.

ANNULAR SHADOWS: ARE THEY CAVITIES OR SPONTANEOUS PNEUMOTHORACES?*

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THE use of the term "annular shadow" has led to a great deal of confusion, and, until its significance is settled, promises to interfere seriously with proper therapy of lung cases which show these shadows. If they be pleural manifestations of localized spontaneous pneumothoraces, they are not of very great importance, but if they can be proven to be lung cavities, as the writer believes them to be, the lighter interpretation of their significance,

FIG. 1. Annular shadow showing opening into bronchus; posterioranterior position.

placed upon them by Brown, Sampson and Heise[1] and a host of followers, is a grave matter. It will delay adequate therapy in many cases in which the shadows do not disappear as the surrounding lung condition improves, for no one will deny that cavities of considerable size do close and become healed by the contraction of fibrosis, and by their obliteration and subsequent metamorphosis as a result of artificial pneumothorax.

The differentiation of cavities from pneumothoraces was discussed in 1917 by Fishberg,[2] who used the term "annular shadows" in a generally descriptive way. Evans[3] followed with an address before this society, in 1918, in which he referred to Fishberg's[2] article and to the work of the Saranac School on annular shadows, which he had observed. He accepted their interpretation of localized pneumothorax without question or reservation. The Saranac group, Lawreson Brown, Sampson and Heise followed, the next year, with an article reviewing their own work and

FIG. 2. Lateral plate of case shown in Figure 1.

limiting the discussion to annular shadows seen in roentgenograms showing linear lung markings, but with no central rarefaction as compared with the outlying field. They regarded many of them as being in the interlobar fissure and associated them with the adjacent pulmonary process, owing their formation to the reaction of the pleura to perforation of the lung.

Twelve per cent of a series of over 400 cases showed annular shadows with the roentgenographic features of density, and linear markings similar to those of the lung field surrounding the shadow, and in contrast to the other annular shadows with central rarefaction and without lung markings.

*Read at the Twenty-third Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Los Angeles, Calif., September 12–16, 1922.

Following this article, Amberson[4] of the Loomis Sanatorium presented a review of 50 cases in which he found the type of annular shadow defined by the Saranac group, and in a series of 100 consecutive examinations he reported 20 per cent as showing these shadows. He likewise, accepted them as pneumothoraces, but introduced as explanation of their origin the idea of their being primary pleural lesions of unusual type. Recently he has published further studies of these shadows, which he describes, in one case, as following a resorbing pleural exudate and disappearing as the condition of the pleura returned more toward normal. One of these shadows was not round, but triangular. He presents the fact that clear fluid was withdrawn from such a shadow showing a fluid level, but gives no illustration or description of the lung condition.

FIG. 3. Cavity outline.

FIG. 4. Complete disappearance, six months later, of cavity outline seen in Figure 3.

The next contribution to the problem comes from Honeij,[5] who discusses "cavity formation and annular pleural shadows." He, too, accepts without question the localized pneumothorax interpretation, but injects a third idea of the pathology, which certainly adds to the confusion. He divides annular pleural shadows into true and false. True shadows, he states, are the result of adhesions following inflammation. "Under certain conditions"—what they are is not stated—"there is an oval or circular arrangement of the pleura to the formation of which the movement of lung and thorax contribute, resulting in the formation of central or local pseudo emphysema." The area around is the more firmly attached organized pleura.

The final contribution comes from Barlow and Thompson,[6] who, in a preliminary report on the subject, say that pneumothorax is the rule rather than the exception in lung tuberculosis, and assume that "annular shadow in the pleura is one of the easily recognized indicating signs of pneumothorax." Even if they mistake cavities for pneumothoraces, they are still wrong in claiming pneumothorax as the rule in lung tuberculosis, as the records of any good roentgenological laboratory connected with a public hospital or sanatorium for advanced cases will disclose. In our own 800 cases at Arequipa Sanatorium, largely early ones, the total number of distinct spontaneous pneumothoraces does not exceed 3 per cent of the patients, of whom for the past eleven years we have had roentgenological studies, often six to ten plates of many of the patients. Even Amberson does not get the percentage of the annular shadow cavities which he calls pneumothoraces over 20 per cent. Barlow and Thompson's further statement that "annular shadow in the pleura is one of

the easily recognized indicating signs of pneumothorax" is also wrong, unless limited by the words "in good *x*-ray plates." In my experience with the oldest clinicians in the country, these so-called annular shadows are peculiarly likely to give no physical signs, and, unless of considerable size, or not draining freely, they are very commonly overlooked in physical examinations.

A brief critical review of the contentions of these supporters of the pneumothorax interpretation may clear the situation.

FIG. 5. Dec. 24, 1921. Annular outline between third and fourth ribs, anteriorly, showing opening into bronchus.

No autopsy examinations of their own or of others support their contention, and the pathological explanation by Amberson and Honeij of the origin of these shadows is against known evidence, and seems made to fit a theory.

Evans criticizes Dunham's[7] interpretation of some of his stereo-clinic pictures and sweeps the soundness of Dunham's studies aside without a single logical argument or substantiated deduction. To quote Evans, "Dunham failed to show that this question had been discussed, and, in fact, several cases described as showing cavity formation were in reality cases of localized air pockets." Evans continues: "In making a report on a chest examination last fall, we noted an annular shadow which we described as atypical of a cavity, but no suggestion was made that it might be a localized pneumothorax. There was a small amount of fluid present in the circumscribed area, this supporting the view that the condition was one of cavity formation. Plates made several weeks later of the same case showed a marked diminution in the size of the supposed cavity and showed the absence of fluid. This of necessity suggested that our first interpretation was an error."

Evans does not make clear why several of Dunham's cases were "in reality cases of

FIG. 6. Jan. 16, 1922. Same case as in Figure 5, after 600 c.c. of air.

localized air pockets." No man in this country has studied chest plates and autopsy findings of cases more faithfully than Dunham, and in a personal communication to Burnham he has said that he never saw an annular shadow which he diagnosed as cavity which was not a cavity at autopsy. Dr. Evans does not give his reason for calling the cavity in his examination quoted above "atypical." Certainly the small amount of fluid present could not be atypical of cavity, nor could its absence a few weeks later. If this be admitted, then Evans was swung over to a new interpretation of his findings by the sole fact that the shadow showed a "marked diminution in size," which he considers common in localized pneumothorax and uncommon in cavity. To this we cannot agree.

The real responsibility of advancing the pneumothorax idea as an interpretation of annular shadows rests with Brown, Heise and Sampson of the Saranac School. As that institution holds the leading place in the country for the teaching of clinical aspects of tuberculosis, a grave responsibility rests upon them if it be proven that their contention is wrong and that their so-called annular shadows for practical purposes are never pneumothoraces, but are cavities. The proof that they are cavities the writer of this paper hopes to make so strong and convincing that it will put the matter forever at rest, and serve to undo some of the harm that has been done by their paper, as has been shown by its too ready acceptance.

FIG. 7. Jan. 24, 1922. Same case, after two more air introductions totaling 1,200 c.c.

FIG. 8. Feb. 16, 1922, after further collapse.

The fact that many of the sanatoria in the east belittled the value of *x*-ray for years has prevented, possibly, their collecting large series of plates in which the pathological conditions of the lung showed graphically the changes which they had been accustomed to describe clinically. Had serial plates been the rule in studying cases, the clinicians would not have fallen into the error of belittling the value of their own examinations and depended so entirely on a roentgenologist's interpretation of a clinical condition. This point is illustrated by Figures 1 and 2, showing annular shadows which were diagnosed as cavities in 1912, and what has happened to them in the ten years that intervened. Figures 3 and 4 show the enormous distortion of the thoracic contents in another one-sided case, where the right lung was once honeycombed with cavities. In both of these cases, adhesions prevented successful closing of the cavities by artificial pneumothorax, although the partial collapse and subsequent diffuse adhesive pleuritis probably assisted in the retraction of the diseased lung and the partial closure of the cavities. It has been our experience that cavities as large in diameter as 3 or 4 and even 5 cm. can close in a relatively few months if conditions are favorable. The fact must have presented itself to everyone who has studied the expansion after pneumothorax, that the diseased part of the lung expands more slowly than the unaffected part. In other words, without the disease there is less tendency to the formation of fibroid tissue, and it is by fibrosis that contraction of the cavities takes place. Adhesive pleuritis helps by pulling over the heart and mediastinum and raising the diaphragm. The adhesions seem to take in the slack from the lessened movement of the affected side.

Against Honeij's views much can be said. He is vague on the proposition of just what he accepts as annular shadows,

and too much of his explanation of size, type, location, wall thickness, shape, size in relation to healing, etc. of cavities, we cannot agree with. It is hard also to fathom the idea advanced regarding the relation of "true annular shadows" and the breaking down of lung tissue. He says: "It is true that the two conditions may co-exist, but equally true that pleural changes may be secondary to cavitation, or vice versa. *Consequently*, if a case is presented in which cavitation is accompanied by a pleural annular shadow, it is generally impossible to differentiate the two conditions." If Honeij means what he says, he admits that a cavity may assume all the characteristics that he ascribes to localized pneumothorax. That is exactly the writer's contention, and Honeij's statement reminds him of the story of the little boy who answered the question as to authorship of the Odessy by saying that it was not written by Homer but by another man of the same name. There is a further vein of humor in his final admission that "one must determine whether the disease is sufficiently advanced to make diagnosis of cavity probable. . . . If physical signs are insufficient to establish the diagnosis of cavity, the assumption is warranted that the annular shadow is due to pleural inflammation." And yet he quotes Norris, Ribadeau-Dumas and others as to silent cavities and the unreliability of signs in general.

It would be unfair to medicine to close this argument without making quite clear that this controversy concerns lesions in the lung giving either no physical signs or very uncertain ones, in the vast majority of cases. Pathognomonic signs of cavity apply to large cavities superficially placed, or containing fluid through which entering air must pass, or in which the air is at times under conditions of tension giving characteristic sounds. The great frequency of cavitation and the method of its development by coalescence of honeycombed areas resulting from softening can be followed easily by serial *x*-ray stereoscopic plates. Screening gives no definition, but admits of study of an expansile and contractile movement of a cavity wall with cough, and flat plates are very often misleading. Even physical signs have to be weighed with the greatest care. In our experience, the increase or appearance of rales after cough following expiration is one of the most dependable signs, when weighed with impaired resonance, but it may be so simulated by pleural sounds as to make differential diagnosis dependent on the best of *x*-ray plates.

FIG. 9. Feb. 23, 1922. Complete collapse prevented by apex adhesions. Cavity outline markedly reduced in size and showing fluid level.

FIG. 10. Same case six months later. Cavity site marked by dense scar.

In spite of the frequency of cavitation without pathognomonic signs, and in spite of the inability to depend on our most classical signs, and in spite of the great adjunct to interpretation, their own writings testify.

The reasons for believing that the interpretation of certain, or any, annular

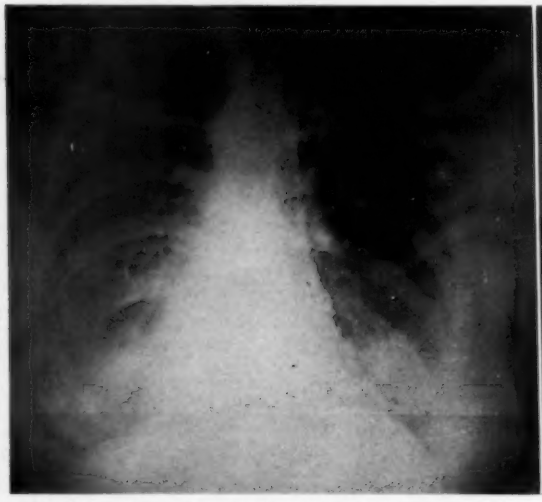

FIG. 11. Two "annular shadows" at right apex. Plate taken in 1913. Upper cavity under the clavicle, lower one from the center of the hilus out toward the upper axilla.

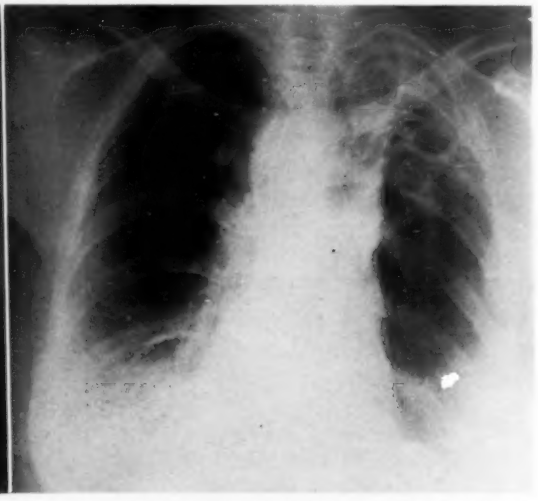

FIG. 12. Same patient, Oct., 1922, nine years later showing deviated trachea, high diaphragm, heart and mediastinum distortion. Cavities still present in markedly contracted right upper lobe.

contribution of the *x*-ray plate, physical examination is and will undoubtedly remain in the hands of well-trained physicians, the best means of determining

shadows as localized pneumothoraces is wrong are these:

1. In a critical examination of many hundreds of such cases, no such shadow,

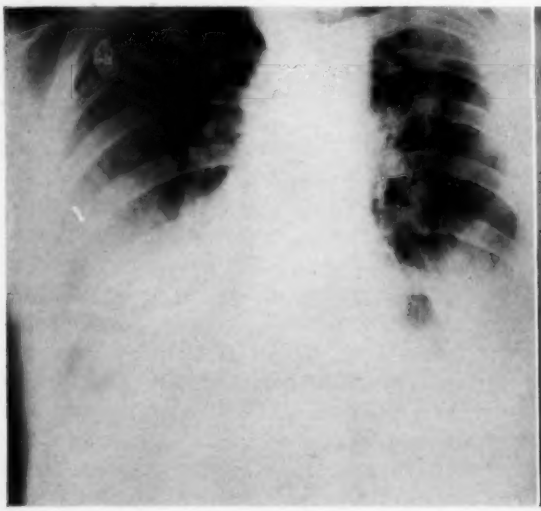

FIG. 13. Multiple cavitation in right upper lobe. Pleurisy at both bases. Plate made in 1917.

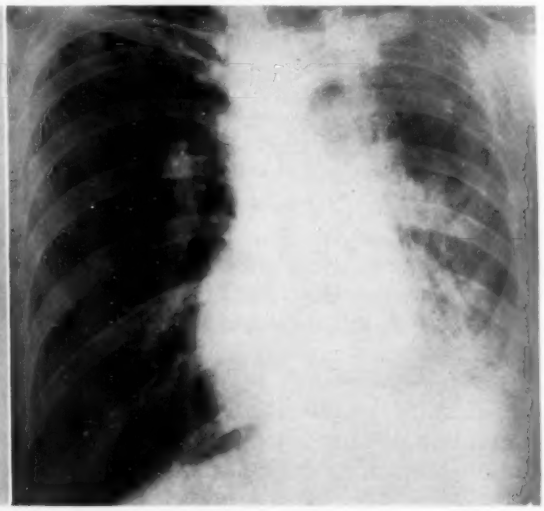

FIG. 14. Same patient two years later showing contraction on the right side. A clearing up of the pleural condition in the left lower lobe.

degree of activity and of interpreting progress. That skilled physicians are the ones who should appreciate most of all the graphic methods of radiology as an

when subjected to one or more of the following tests, has been proven to be anything but a cavity. While not denying

that a localized pneumothorax *may* show as an annular ring form, the writer has never met such a spontaneous pneumothorax, although he has seen a good many recent ones in *x*-ray plates, none of which pneumothoraces was to be confused with cavities, and none was round. Furthermore, stereo plates of artificial pneumothorax where adhesions have limited the air to 100 c.c. or less, have never assumed the appearance of these annular shadows (Fig. 1).

2. Anteroposterior and lateral views of these conditions show them to be equally round in both directions, which could be true only if they were cavities (Figs. 5 and 6, etc.). Apex cavities cannot be shown this way, owing to intervening structures.

3. Artificial pneumothorax will first compress and then collapse these shadows and move them from their original positions, which would be impossible if they were in the parietal pleura (Figs. 7, 8, 9, 10 and 11).

4. If unilateral, and found sometimes to contain fluid, the shadow may be studied by *x*-ray in the morning after the patient has spent the night on the affected side, *before* the cavity has emptied itself and *after* postural effort at emptying. Pneumothoraces containing fluid will not empty. Matson has injected methylene blue into such an area and had it expectorated at once.

5. Finally, stereograms such as we have been fortunate enough to get, taken in $\frac{1}{60}$ second each, so that there is no interference with outlines due to motion transmitted by heart action, give a detail never secured before in lung plates, and show not only the perspective of these shadows, and that they are cavities, but orient them in their relation to the chest wall, show their connection with bronchi, and how they increase in size by coalescing with adjacent and often very small cavities or by including softened areas which border on them.

If any or all of these tests are applied to "certain appearances in *x*-ray plates of the chest" not recognizable by clinical signs, we contend that they will be found to be cavities, and that for prognostic and therapeutic purposes they must be so regarded even when satisfactory means of proof are not available. At least, let us apply the test and not give so grave a condition as a cavity an interpretation that might end disastrously to the patient. Consider all annular shadows as cavities until diligent investigation proves that they are to be better interpreted some other way.

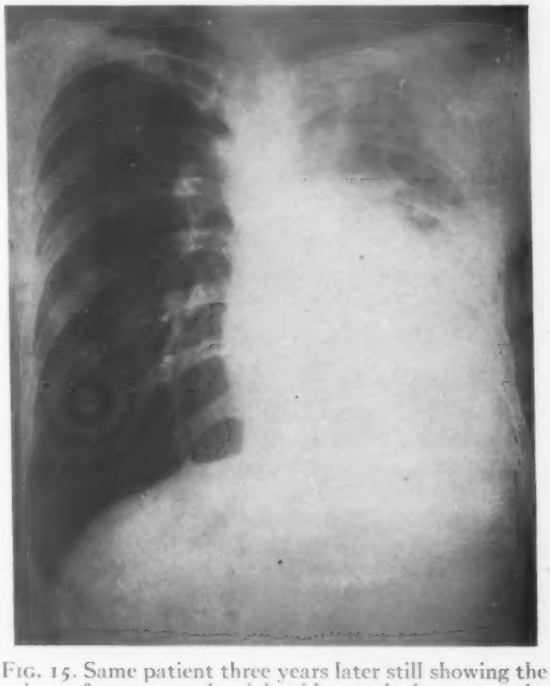

FIG. 15. Same patient three years later still showing the heart far over on the right side; marked curve to the right of the trachea. Right diaphragm pulled up very high. Interspaces markedly narrow. Cavities at right apex still unclosed.

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DISCUSSION

DR. BURNHAM. I accept what Dr. Brown said regarding the fact that annular pneumothorax does not exist. We are all familiar with localized pneumothorax. It is never annular in contour.

Four cases of so-called annular pneumothorax have been autopsied, and they proved to be cavities.

Annular shadows, the center of which have the same density as the surrounding lung, with linear lung markings, are a sign complex not difficult to keep in mind; but if we wander away from the subject to talk about localized pneumothoraces not of this type, it complicates things.

The question of the walls of the cavities has received very little consideration from any one. During the past year Dr. Brown and myself have tried to find out about the formation of the cavity walls as regards thickness. Why do we see thin and thick ones? In our work we were struck right away by the fact that in these thin-walled cavities there was, at the bottom of each, a large opening into the bronchus. We thought that was a factor in the formation of thin walls: viz., easy drainage, with a large opening in the dependent part of the cavity. That is presented for what it is worth. Many other factors must enter into the formation of the wall.

I would like to ask the president if he would call on Dr. Matson to discuss the paper.

Dr. Evans. It is evident to me that I am on the defensive. Some five years ago I made my first observations on this subject. A year later I studied the work with Dr. Thompson, and the same fall I published my observations.

In spite of the fine paper this afternoon, I am still unconvinced that there are not annular shadows which are not cavities, for these reasons: First, I have been able to tell in advance when a shadow would appear, and have studied serial plates in which I have first noted a superficial lesion and secondarily noted an annular shadow which was round, and which represented in general appearance the shadow shown this afternoon. I believe all these shadows which I have seen were cavities, with possibly one exception. In spite of that, there are, in my opinion, annular shadows which are not cavities. In the second place, we have considered the associated pathology. I have seen annular shadows in cases of so-called apical tuberculosis in which you would not find cavity formation because it is acute. I have seen the same lesion in so-called peribronchial tuberculosis. In that type you would not expect to find cavity formation. On the other hand, we have studied cases serially, and have observed that those annular shadows do change rapidly in size and outline.

I am classifying now 2,000 chest cases and I hope to get positive figures. Burnham referred to the post-mortem finding in a case in which there was found an annular shadow which he said was localized pneumothorax. That is reported in the *Journal of Tuberculosis*. As a further proof that some of the shadows are not cavities, I believe that some workers have been able to withdraw a serous fluid from an annular shadow. Some cavities have been injected and the patient has been able to cough up the solution placed in the cavity.

I think the matter is important and that we appreciate the discussion. Since my formal report, I am frequently called upon to say that I cannot properly classify annular shadows: they may be cavities or localized pneumothoraces, and the subject requires further study.

Dr. Matson. The question of annular shadows and cavities is one which has concerned me a great deal during the past few few years, especially after reviewing the literature. The frequency of spontaneous localized pneumothorax is much more frequent, according to the men describing this condition, than we have found in our own experience.

The question of differentiation between closed pneumothorax and cavity is very easy because it requires only the introduction of a needle. If one introduces atmospheric air in this cavity, it will escape through the bronchus. If we inject weak dilution of methylin blue, it is expectorated. But the question of differentiation between localized closed pneumothorax and circular lesion is much more difficult. We have seen in roentgenograms many times what we thought to be a circular lesion and would prove it to be a cavity although there were no physical diagnostic evidences of cavity. We all know that cavities exist in the lung.

We know that in most instances in a cavity, if close to the chest wall, there is always a patch of adherent pleura over the side of the cavity, and we have watched the separation of these bands.

Dr. Manges. This is a very interesting situation. The original work of Sampson, Heise and Brown seems to have been very well done, at least from the roentgenographic point of view. And now Dr. Brown and Dr. Burnham, men of renown and ability, take the stand that the interlobar annular shadows do not exist. I think we ought to look upon the pros and cons with at least a fair thought to both sides. I am convinced that they are not of frequent occurrence, but I feel equally certain that they do exist, and that they may or may not contain air; or, in other words, that they may not even be pneumothoraces.

There are questions that may be asked of either side. First, if it is an interlobar pneumothorax from a perforating tuberculous focus,

why does not a complete pneumothorax develop? Spontaneous pneumothorax is supposed to occur in this way and should produce sudden symptoms. On the other hand, if these large cavities are always the result of abscess in the lung tissue, why do we not always get a history of acute illness with sudden expectoration of a quantity of pus, or roentgenographic evidence of the formative stage of the abscess? And why is it that in many of the cases there is very little other roentgenographic evidence of tissue change in the lungs?

Again, granting that encapsulated pneumothorax is a pathologic entity, what is there to prevent the formation of exudate, or of its communication by way of ulceration with a bronchus? Why should not such a cavity have depth in all diameters, or be more or less spherical? And why should it not respond to treatment by compression, the same as any other cavity?

Thinness of the limiting wall offers further opportunity for argument. I am sure I have seen such shadows that were incomplete, or with part of the wall so thin as not to be recognizable in shadow. I have one case in mind in which there were numbers of these annular shadows. Some of them, I believe, represented cavities in the lung tissue; others were not truly annular, had little if any depth, and, on the left side especially, were distributed from above and behind, downward and forward in the line of the interlobar fissure. (Stereoscopic slides of this case were sent to Dr. Heise and he called my attention to distribution mentioned above.)

I have seen at autopsy, and especially during the flu epidemic of 1918, instances of localized pleuritis between the lobes, the lesions being small or large, but often circular in outline, with well-organized adhesions at the circumference and more or less plastic exudate within the ring. It is entirely reasonable to suppose that, later on, such lesions would produce annular shadows on the roentgenogram.

I do not believe that the annular shadow of interlobar origin is very frequently seen, but I do believe in its existence. Further, I believe that many of the cavities with thin walls in the lower two-thirds of the chest, in the absence of advanced pathology in the upper third, are bronchiectatic, and not true abscess cavities.

Dr. Bowen. This subject is exceedingly interesting to me because of working with Norris for two years. In formulating a rather set type of instructions in roentgenology for classes, he just recently came to the conclusion that we should leave out any reference to annular shadows, for the reason that we then considered the data a controversial subject. Norris has had an exceedingly large experience in watching pathological post-mortem examinations, and is sure he has never seen one.

On the other hand, there is a case that had even more of these than the one which Dr. Brown called to our attention, and while his impression seems to bear out the fact that these could hardly have been pneumothoraces, this man could not interest the tuberculosis dispensary in his case, even after we had shown the shadows. He seemed to have absolutely no physical signs, and I think there were seventeen of these shadows that we outlined very completely.

Dr. Brown (closing discussion). I should not like to be quoted as saying there is no such thing as an annular formed pleural shadow due to pneumothorax.

I had a case recently of spontaneous pneumothorax with symptoms of localized pleural irritation only, and had the roentgenologist point it out to me, because I had not found it on physical examination. It was not like anything that these people have called localized spontaneous pneumothorax of the annular shadow type, yet you all would recognize it as a nice limited pneumothorax in the plate. In the first place, you can see that the edge is not round, although it will probably get round in time from the effects of pressure. When small pneumothoraces first appear, they are never round, whether produced by lung rupture, or artificially, by air introduction. I see a great many of them in plates taken after the first artificial pneumothorax, where adhesions limit the diffusion of air, and I have never seen one that looks like a cavity. It is the old spontaneous pneumothorax that has the big thick wall and is relatively round, that is sometimes hard to tell from a cavity.

In regard to the question of cavities getting larger or smaller; if you will follow a case with small cavities, with plates made monthly, you will often see a change in the size of the cavity. Often you can see that it is getting smaller, and finally it disappears completely as a cavity. In unfavorable cases, it may get a thicker and thicker wall and grow slowly and symmetrically larger, or it may increase in size by incorporating other adjacent cavities or softened areas.

A great many people with bad cavities work up to the day they are put to bed never to get up. The undermining of vitality may be a gradual thing, and people do not recognize the seriousness of the situation until they are told what is wrong with them. In these progressive cases, annular shadows, properly studied, reveal promptly their true character.

CASE OF BULLET IN HEART WITH RECOVERY*

BY F. C. SWEARINGEN, M.D.

POMONA, CALIFORNIA

MR. M., aged forty-five, had influenza in December, 1918. During his convalescence his wife died of influenza pneumonia. On Feb. 17, 1919, being despondent, he attempted to take his life, after which he asked the telephone operator to summon his family physician. He was found lying, face down, inside his front door. He was unconscious, deeply cyanosed, with respirations stertorous, no pulse perceptible at wrist, and no heart tones to be heard over the cardiac area. Examination showed a bullet wound in the fifth intercostal space one-half an inch to the left of the sternum.

FIG. 1. Case No. 7075. Lateral view showing shadow of bullet within heart shadow.

FIG. 2. Case No. 7075. Anteroposterior view showing shadow of bullet within heart shadow.

The patient was placed on his back and given 1/60 gr. of strychnine sulphate hypodermatically. In a few minutes his respirations and color improved, and he soon began to regain consciousness. He was removed to the hospital where examination at 12.50 p.m. showed no radial pulse, very faint and indistinct heart sounds over the heart, and cardiac area of dulness very much increased. The patient was entirely conscious and complained of some pain in the left chest which was not aggravated by deep respirations. There was no nausea, no expectoration, and no cough at this time or at any time during his illness. The wound of entrance was dressed, and no wound of exit could be found.

At 1.35 radial pulse was perceptible for the first time, but not strong enough to make it possible to count pulse, nor were the heart sounds clear enough to count pulse with the stethoscope.

At 1.50 the patient became very restless and complained of severe pain in the chest.

At 3.00 the radial pulse was strong enough to count, regular in rhythm and registered 98 per minute.

The patient rested fairly well during the rest of the day and slept some during the night. The following morning he took a little liquid food.

During the next six days the pulse varied between 80 and 100, was always regular, and gradually increased in volume. The heart sounds also gradually improved in clearness. From the sixth to the twelfth day the pulse ranged between 70 and 90,

* Thesis presented leading to membership in THE AMERICAN ROENTGEN RAY SOCIETY, 1921.

the cardiac dulness decreased and the heart sounds became clear and distinct.

Improvement was uneventful from this time, and the patient left the hospital on March 4, 1919.

Examination of the house immediately after the shooting showed that the patient was lying on the bed at the time of the accident, his revolver, a 32 caliber, and some small pieces of a vest-button which was shattered by the bullet being found on the bed. From the bed he walked 15 ft. to the telephone. Fresh blood-stains on the directory showed that he had attempted to find his doctor's telephone number. After calling central and asking her to summon his physician, he walked another 20 ft. to reach the front door, where he was found, and where he evidently collapsed.

On March 11, 1919, the patient walked into my office seeking a roentgen examination. He was at that time experiencing some difficulty in the muscular action of the 4th and 5th fingers of the left hand, and his physician thought perhaps the bullet had traversed upward and located in the region of the cervical plexus.

The wound of entrance was still plainly visible. The patient was placed in the anteroposterior position before the fluoroscope, and immediately a foreign body was noted just posterior to the wound of entrance and within the shadow of the heart approximately midway between the right and left borders and approximately 2 cm. below the greatest diameter of the heart. There was an oblique movement of the foreign body with each impulse of the heart.

The patient was then rotated in both directions with the shadow of the foreign body always in view. At no time did it pass out of the shadow of the heart.

In the lateral view, the shadow of the foreign body was situated approximately 1 cm. anterior to the mid-axis of the heart shadow.

Owing to the movement of the heart, roentgenograms were taken with the aid of the intensifying screen, plates being taken in both the anteroposterior and the lateral positions. Each of the plates thus exposed shows the shadow of the foreign body well within the shadow of the heart.

A second examination was made on Sept. 14, 1919, six months after the accident, with the same findings as the first, except that the heart shadow was perceptibly smaller. A recent examination reveals no change, and the man is at present working at moderate manual labor in a fruit packing house, apparently enjoying the best of health.

I am indebted to Dr. A. L. Weber of Cucumonga, Calif. for assistance in the history of this case.

THE LUMBAR TRANSVERSE PROCESSES

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ST. LOUIS, MISSOURI

IN following a routine for gastrointestinal examinations—wherein a preliminary radiogram of the patient was taken in anteroposterior direction—of the kidneys, gall-bladder and lumbar spine, there have frequently been observed solutions of continuity of the transverse processes of the lumbar vertebrae, usually unilateral, but at times bilateral. These were diagnosed as congenital anomalies, and the question of pathology was never brought up. These cases were never reported as showing pathology of the spine in the preliminary radiogram.

FIG. 1. Unilateral lumbar rib simulating fracture of the transverse process of the first lumbar.

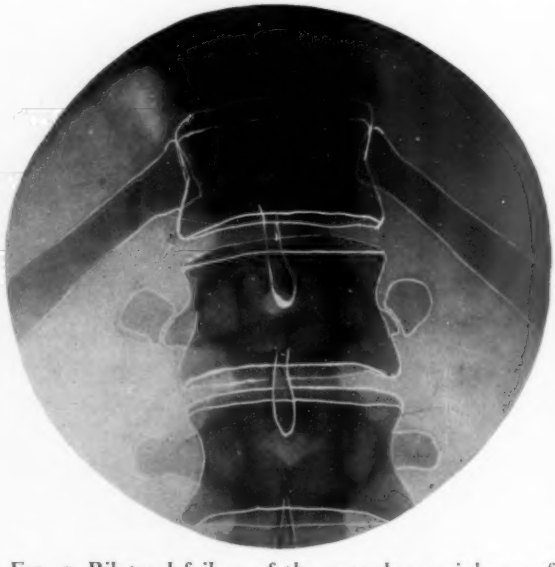

FIG. 2. Bilateral failure of the secondary epiphyses of the first lumbar to fuse. Before retouching of film the condition very closely simulated fracture.

On one occasion, a case reported as negative on the preliminary examination was reported back as having had a roentgen-ray diagnosis of fracture of the transverse process of the first lumbar vertebra about three years previously. On re-examination of the film, it was found that the transverse process was divided into an inner two-thirds and an outer one-third. The line of division was sharp, somewhat curved, with the concavity directed laterally. The margins of the bone were smooth and clearly outlined, and there was no calcium-salt absorption present. In view of the history of an injury to the back in this case, comparison of the radiogram with other cases showing similar divisions of the transverse processes was made. No differences could be discerned of sufficient importance to indicate that the solution of continuity of the transverse process of the first case was of different character from the divisions noted in the other cases which had no history of back injury and were found only on routine preliminary roentgenograms.

A review of the development of the vertebrae in the lumbar region shows that in an embryo of about 15 mm., chondrification is already taking place. Each vertebra is provided with a rib process, but there is no separation between the rib and the primitive vertebra until late in the cartilaginous stage. In the cervical, lumbar, and sacral regions, the processes remain attached to, and become parts of, the vertebrae, but in the thoracic region, they grow around the body wall to form the free ribs. The costovertebral articulations

are produced by absorption in the matrix between the ribs and the vertebrae, the surrounding mesenchyme giving origin to the costovertebral ligaments.

The rib process and the growing transverse process are at first united by a continuous blastema. This is absorbed as anastomoses are established between the segmental arteries, but between the end of the process and the rib a joint cavity is formed and the surrounding mesenchyme gives rise to costotransverse ligaments. The neural process, which is situated immediately behind the costal process, gives rise to the articular and the transverse processes. Centers of ossification appear in the body of each vertebra, and following this, a center appears in each half of the vertebral arch. Ossification of the arches begins in the upper cervical region about the seventh or eighth week of fetal life, and gradually extends down the column. The centers of ossification first appear in the situations where the transverse processes afterward project, and spread backward toward the spinous processes.

At birth, the vertebra is in three pieces, the body and the two halves of the vertebral arch. During the first year, the bodies of the upper cervicals join the arches. In the lower lumbar region, union takes place about the sixth year. The ossification takes place very slowly, and bone replaces the cartilage fairly late in life. At about the age of puberty, secondary centers of ossification appear in each of the cartilages that cover the ends of the vertebra. Secondary centers also appear in the cartilage on the tips of the spinous and transverse processes, and in the lumbar vertebrae one appears also on the tip of each articular process. The secondary centers unite with the vertebrae at any time between the sixteenth and twenty-fifth years.

Occasionally, anomalous development takes place, the costal process of the lumbar vertebra developing to a somewhat greater degree than usual and becoming separated from the vertebra. It fails to fuse with the transverse process and remains entirely separate as a lumbar rib. Occasionally, also, the secondary epiphysis on the transverse process of the first lumbar vertebra fails to unite with the transverse process, and produces a condition which simulates a fracture.

At times, difficulty in diagnosis may arise as to the presence or absence of a fracture when there has been an injury to the back. A lumbar rib should give no difficulty in diagnosis, provided the transverse process on the opposite side is shorter than the transverse process and lumbar rib on the suspicious side. The other evidences of fracture of the bone will also be absent. Where the secondary epiphysis has failed to unite with the transverse process, and the history is that of an injury, the greatest difficulty will arise. Here it will be seen that the lateral fragment is smooth, well outlined, shows no decalcification and has none of the characteristics of a fracture. Re-examination of this transverse process a few months later will show absolutely no change in the condition. One case observed about a year later showed the condition to be exactly the same as during his first examination. The average age of the patients was about thirty years.

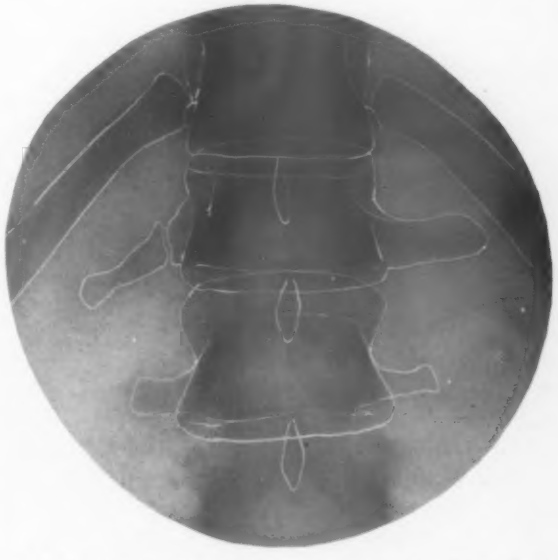

FIG. 3. Unilateral lumbar rib simulating fracture of the transverse process.

The anomalous development already described is not at all rare; in fact, it is quite common. It has been found to occur in about 5 to 10 per cent of all gastrointestinal patients examined over a period of about six months. The situation of the

transverse process of the first lumbar vertebra is such that it is brought out on the radiogram with difficulty, and it is probable that faintness on the film is the chief cause of this condition being overlooked so frequently, unless there has been an injury to the back, and a special search is made for a fracture. It would be interesting to know how frequently this condition would be diagnosed as fracture. Radiograms of fractures of the transverse processes show a tendency to separation of the fragments, decalcification of the bone, irregularity of the adjacent margins of both bones, and other evidences of attempt at either absorption or union between the fragments.

CONCLUSIONS

1. The transverse processes of the lumbar vertebrae, especially the first lumbar, very frequently show anomalous development.

2. The unilateral lumbar rib is easily differentiated from fracture, unless the rudimentary rib is very short. Then differentiation must be made by the appearance of the bone, the situation of the lesion, and the lack of injury to neighboring parts.

3. Unilateral or bilateral failure of the secondary epiphysis to unite with the transverse process, while probably the least frequent of the two conditions, will, at times, also be difficult to differentiate if found after injury.

BOOK REVIEWS

RADIUM THERAPY. By Frank Edward Simpson, A.B., M.D., Professor of Dermatology, Chicago Policlinic; Adjunct Clinical Professor of Dermatology, Northwestern University Medical School; Attending Dermatologist to Mercy Hospital, Alexian Brothers Hospital, Henrotin Hospital, etc.; Former President American Radium Society; Former Vice-Chairman, Section of Dermatology and Syphilology, American Medical Association; Director of the Frank Edward Simpson Radium Institute. Cloth. Pages 388, with 166 original engravings. St. Louis, 1922.

This most timely and welcome contribution to the literature of radium therapy is worthy of a place on the shelf not only of every radiologist but of every physician and surgeon. Radiotherapy has developed to the point where it should enter into the practice of every medical man, no matter what his specialty. Whether or not he actually applies the radium will depend on the circumstances and the effort which he has made to fit himself especially for the work; but the field of usefulness of this wonderful salt is ramifying into nearly all fields of medical activity to such a degree that it is incumbent upon every physician to familiarize himself with the general principles of radiotherapy, or at least to have upon his desk a book of reference, for ready use. This need is admirably met by the work under review.

The book consists of twenty-one chapters and an excellent bibliography. Every feature of radium work, both technical and clinical, is included, and the author has written his book in such style that it is a pleasure to read it. It easily takes first place in publications on radiotherapy up-to-date.

JAMES T. CASE.

TREATMENT OF CARCINOMA OF ESOPHAGUS BY DEEP X-RAY THERAPY*

BY A. H. PIRIE, M.D.

MONTREAL, CANADA

THIS paper is not a record of successes, but rather an inquiry into failures. It is based on the unsuccessful treatment of 8 cases of carcinoma of the esophagus during the past year.

Three cases were treated by inserting a tube of platinum containing 10 mg. of radium into the center of the growth for thirty-six hrs., i.e., (360 mg. hrs.,) and applying erythema *x*-ray doses around the growth with 100 kv. and aluminum filter of 3 mm. This treatment failed in 3 cases, and all the patients are dead. Three other cases did not return for treatment and 2 cases have been treated with *x*-rays from 200 kv. and one is still alive.

Carcinoma of the esophagus has, up to the present, been a lingering form of death. No treatment has done anything to cure it. The surgeon prolongs the patient's miserable existence by making a gastrostomy and feeding him through the artificial opening. I believe there are a few successful cases on record of an artificial tube of skin having been made, short-circuiting the carcinoma and so keeping the patient alive for a year. These measures do not cure the carcinoma, and the only possible hope of cure is held out by the use of radium and deep *x*-ray therapy.

I made use of radium in 3 cases mentioned by placing a tube of radium in a small catheter and leaving it in position in the center of the carcinoma. The radium is in a platinum tube which can be seen with *x*-rays, and thus its position in the middle of the growth can be easily determined. The cases in which I have done this have swallowed better after the treatment than before, but eventually all died. The improvement in swallowing I have ascribed to mechanical dilatation rather than to the effect of the radium.

Since deep therapy came in I have attempted to influence the growth by means of *x*-rays produced by 200 kv. The first case was one of carcinoma of the esophagus involving the larynx. Three and a half erythema doses were given by cross-fire to the neck over the carcinoma.

Two weeks after the treatment the patient could swallow meat. After a month, however, she had difficulty again in swallowing.

One month after her first treatment the skin of the neck over the region of the growth was red and peeling in front on the right side and behind. On the left side half an erythema dose had been given and the skin was not affected. The hair fell out at the lower part of the scalp behind. This patient had received 3½ erythema doses by cross-fire. She had a small neck and I calculated that 240 per cent of an erythema dose had been given to the growth. Two months after the first treatment, the patient did not appear to be improved. She was very weak and could swallow liquids only. Every time she swallowed, a little liquid went into the larynx and caused her great distress. By bending forward she could swallow a little better than in any other position.

Treatment was begun four months ago, when she had reached the stage at which liquids would not pass down the esophagus. When a patient reaches this stage he usually will not live longer than three weeks. This case, however, recovered so far that she was able to swallow meat two weeks after the first treatment. Today the condition of the patient is bad. She swallows, but is in a very low condition and is likely to die soon.

This case was particularly suited for deep therapy. The diagnosis was made by an experienced laryngologist. The growth was in an ideal position for cross-fire. The neck was small and fairly long. Three and a half erythema doses were given. Erythema and peeling followed on three areas and hair fell out. Therefore it was plain that the patient had received the dose. The rays were filtered through ¾ mm. of copper on each occasion and the skin focus distance was

*Read at the Midwinter Meeting of the Eastern Section of THE AMERICAN ROENTGEN RAY SOCIETY, Atlantic City, N. J., Jan. 25–27, 1923.

16 in. The kilovoltage was 200 to 210 and 5 ma. Time one hour for erythema dose. The half erythema dose was given to the left side of the neck with a lower voltage and its effect is not likely to be great in comparison with the doses given with 200 kv. The treatment was repeated six weeks after the first treatment. Two months after the first treatment the patient could swallow liquids, but was much distressed while doing so because of the liquid entering the larynx. I tried to get her to allow a tube to be passed so as to prevent liquid from entering the larynx, but she preferred to die rather than have that. The failure in this case was due to ulceration into the larynx, and a tube for the passage of food would have been the only remedy.

The rays may have influenced the growth, but the result has been a failure. I would not have repeated the dose if the patient had been doing well, but she was not doing well six weeks after the first treatment. The ulceration into the larynx may have been the cause of death.

My calculation of the doses she received is 240 per cent erythema dose on the first occasion and the same on the second occasion. The skin of the neck became very brown, but there was no vesication.

It may be the carcinoma received an overdose, yet a rodent ulcer heals well after giving three erythema doses, so why not give as much to carcinoma? My failure, however, does not discourage me. I have faith in the treatment and shall advocate it, until I either make a success of it or find out the cause of its failure.

I advocate it because it is the only case of early carcinoma that I get to treat and feel justified in treating. Any other early carcinoma should be removed by surgery, but in carcinoma of the esophagus the surgeon is helpless, except to do a gastrostomy.

Dr. Reginald Morton of London reports a case of carcinoma of esophagus treated by deep therapy alive over a year after the treatment. One successful case is sufficient to warrant every case of carcinoma of the esophagus receiving the same treatment.

With a machine that delivers 200 kv., using 5 ma. and ¾ mm. of copper at a distance of 16 in., I find I get a mild erythema in one hour and an intense erythema in an hour and a half. In opposition to every one else, I use a pastille to measure my dose. I use a Corbett's tintometer and lay a pastille on the skin. At the end of an hour's treatment the pastille has colored to the half tint of the tintometer. This is a mild erythema dose. I admit up to 20 per cent error in dose by this measurement, but one must also admit 25 per cent difference of reaction with different patients. If therefore I give 20 per cent overdose to a 25 per cent over-sensitive patient, I shall have given an erythema dose and a half. This will not be serious and will not cause much discomfort. The pastille integrates the dose where it is given, in a way that no other instrument does. I have used the pastille to measure over 1,000 doses during the last year, and I have not yet found it to fail, either for epilation of ringworm or for deep therapy. In every case I lay the pastille on the skin at right angles to the incident ray. Armed with a pastille and a Corbett's tintometer I would not hesitate to go to a strange deep therapy machine and deliver an erythema dose without any previous practice. Although I am assured by the makers of my deep therapy outfit that there is a stabilizer supplied, I find that my milliamperemeter will not stay at 5 ma. for an hour. The change noted is from 4 to 6 ma. The pastille looks after this variation and integrates it.

Another advantage of the pastille is that I can supervise the work of treatment by its means. I have three treatment machines, and at the end of the day my assistants who have done the treatment lay on my desk each patient's index card of treatment with the pastille attached. My assistants estimate the dose from the pastille and note it on the card and I examine each pastille and confirm the dose.

My voltage is measured by balls of 12.5 cm. diameter and it is interesting to note that the E. S. G. between blunt points varies according to this chart. In summer the humidity of the air is higher, and in winter it is lower, as the air is very dry.

Before closing, and wishing to encourage others to use deep therapy for treatment

of carcinoma of esophagus, I present to you a table showing the dose delivered to the esophagus at the level of the 7th cervical, 4th dorsal and 11th dorsal vertebrae, using 200 kv., 5 ma., 16 in. S.F. distance, surface square of 6 in. side, ¾ Cu. filter calculated from Dr. Mutcheller's tables and Symington's cross section anatomy.

AT LEVEL OF 7TH CERVICAL VERTEBRAE

Position	Depth, cm.	Percentage Dose
Front.....	3½	75.99
Side.....	5	71.13 × 2 = 142.26. Total 278.95
Back.....	7½	60.78

AT LEVEL OF THE 4TH DORSAL

Position	Depth, cm.	Percentage Dose
Front.....	6	69.73
Each side..	10	51.58 × 2 = 103.16. Total 254.16
Back.....	7½	60.78

AT LEVEL OF THE 11TH DORSAL

Position	Depth, cm.	Percentage Dose
Front.....	8	60.78
Each side..	10	51.58 × 2 = 103.16. Total 220.10
Back.....	9	56.16

I therefore cannot claim any success in treatment of carcinoma of the esophagus by deep therapy, yet I believe this treatment will be successful and I strongly advocate its use. I urge it because of the results I have had from treatment of carcinoma deeply seated elsewhere. My experience of carcinoma of uterus and adnexa treated with deep therapy confirms the findings of others. It has amazed me as well as my confreres.

I am indebted to Dr. Brooks for making the calculations for the tables, and for the diagrams.

DISCUSSION

DR. LEWALD. There is one case alive ten years after a surgical cure, in which the esophagus was actually resected. I have seen the specimen and have seen the microscopical slides. It is an adenocarcinoma of the esophagus and it is true that that case is surgically cured. The esophagus was resected as far as about the second thoracic space and then a tube inserted into that. The patient masticates the food and it passes into the tube and goes into the stomach through a gastrostomy. An attempt to reconstruct the rest of the esophagus has been advised, but the patient has been so comfortable that she refused to have it done.

About the spark-gap reading: We also noticed that the sphere gap is decidedly off in exceedingly high humidity. At 90 per cent humidity the sphere gap is far from correct.

CARCINOMA OF THE FLOOR OF THE MOUTH *

BY DOUGLAS QUICK, M.B. (TOR.)

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INVESTIGATION of the surgical literature reveals little recognition of carcinoma of the floor of the mouth as a distinct clinical entity. British and American writers have usually grouped it with intraoral lesions in general, and as such it has received no special attention. The French have, for the past fifty years, devoted considerable attention to it as a distinct surgical problem. Verneuil's[1] paper on "Epithélioma des glandes sublingual" in 1871 is the first publication on the subject, and as the title indicates, he considered it primary in the sublingual salivary glands. Margnat,[2] in his Paris thesis of 1877, elaborated on Verneuil's work and in 1886 Feuilletard[3] made Richet's method of operation the subject of his thesis. In 1902 de Robillard[4] wrote a thesis on the operative procedure first outlined by Faure in 1897. Since 1907, Morestin[5] has made several reports on the operative procedures of choice, and, from a surgical standpoint, has undoubtedly given more attention to the subject than other contributors.

Unfortunately, little has been written about the clinical characteristics of the disease. It is with the hope of adding something to this phase of the subject and correlating it with treatment that this paper is presented.

Carcinoma of the floor of the mouth presents a clinical picture so definite that it can readily be recognized, even in late

* Read at the Midwinter Meeting of the Eastern Section of THE AMERICAN ROENTGEN RAY SOCIETY, ATLANTIC CITY, N. J., JAN. 25–27, 1923.

stages of the disease after extension to adjacent parts has taken place. The lesion begins almost invariably in the mucosa of the anterior half of the floor of the mouth and usually just at the side of the frenum of the tongue. The rate of growth is rapid. Potentially malignant growths of long duration, such as are not infrequent on the tongue, are practically never met with in the floor of the mouth. In our experience, all the cases have been of the squamous-cell type. While the papillary form of squamous carcinoma occurs here, it represents only a small minority: the infiltrating type is the rule. The disease establishes itself early and deeply in the musculature of the floor of the mouth, and, since the anatomical arrangement facilitates extension, the depth of infiltration is relatively greater than in any other group of intraoral carcinomas. This feature has added uncertainty to the surgical procedures and is equally baffling in determining the depth to which radium should be inserted. Upon more than one occasion I have thought I was dealing with a deep, fixed, submaxillary lymph-node, only to find at operation the deeply infiltrating base of the primary growth representing what I had thought to be the node. In those growths beginning at the side of the frenum of the tongue, extension to the opposite side is rapid. This creates essentially a "double" lesion. It necessitates a more complicated course in treatment of the primary growth. It makes a wider lymphatic dissemination possible, and if ligation of vessels is necessary, it must be done on both sides rather than one.

A peculiar characteristic of the disease is the infiltration of the tongue from below upward. This is not necessarily accompanied by extensive ulceration of the mucosa of the tongue until late in the course of the disease. Hence, only palpation will reveal it. This tendency to upward extension into the tongue is even more marked than that of infiltration downward along the muscular planes of the floor of the mouth. Possibly the rich blood supply and freedom of motion of the tongue are contributory factors. I have seen a growth of this type show no clinical evidence of backward and downward extension posterior to the molars until ulceration appeared at the base of the tongue at the side of the epiglottis.

These growths usually arise in the mucosa at the inner or lingual side of the floor of the mouth, and this probably accounts for the early medial rather than lateral extension. They do, however, extend laterally both by direct extension in the mucosa and by infiltration along the muscles attached to the lower half of the inner surface of the mandible. When this is complete, the floor of the mouth is solid and invasion of the jaw along the line of muscular attachment is frequent.

While I am opposed to surgical removal of any intraoral carcinoma, I have nevertheless tried to maintain for statistical purposes a fair classification of surgical operability. In our series of over 100 cases, I have not seen more than a half dozen that could be considered operable from a surgical standpoint.

Extension to the lymph-nodes is frequent and early. The submaxillary nodes on the side of the primary growth are most frequently involved. If the growth has extended beyond the midline, the danger of lymphatic dissemination is of course much greater. Next to the submaxillary group, the jugular node of the upper deep cervical chain overlying the carotid bulb is most frequently involved.

The possible contributory causes of carcinoma of the floor of the mouth are interesting. Since the lesion almost invariably occurs in the anterior half of the floor of the mouth, the heat from smoking must be considered. This feature is strengthened by the fact that the disease occurs very rarely in women. Leucoplakia bears the same relation here as elsewhere—that of a chronic irritant. It is, however, relatively infrequent in the floor of the mouth. On the other hand, it is in this location somewhat macerated from constant bathing by mouth secretions and probably more dangerous. The constant motion of the structures forming the floor of the mouth undoubtedly aids in irritating and disseminating the disease, once it is established. The constant bathing by oral secretions and food debris is a source of irritation where ordinary cleansing is neglected.

Bad teeth are a factor only indirectly by contributing to the general uncleanliness of the mouth. Ill-fitting lower dental plates, on the other hand, are a real source of direct irritation.

The treatment of this disease has until recently been largely surgical. Escharotic pastes are not well adapted to use on moist surfaces. The various heat methods have been used considerably, but from a theo-

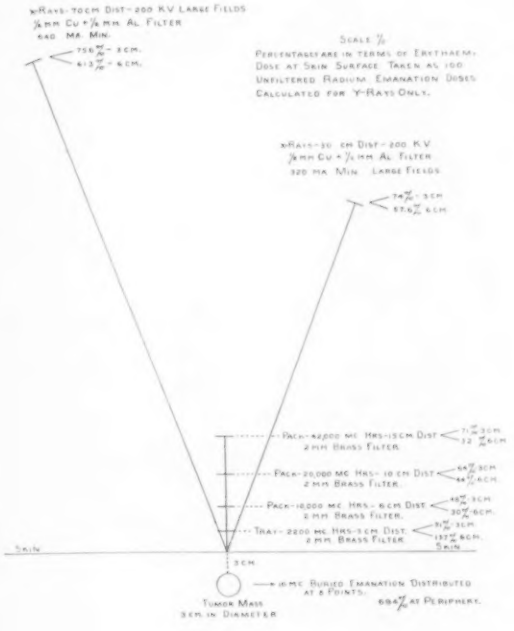
X-Rays- 70 cm Dist- 200 K.V. Large Fields
½ mm Cu + ½ mm. Al. Filter
640 Ma. Min.
75.6% - 3 cm.
61.3% - 6 cm.
Scale ½
Percentages are in terms of Erythaema Dose at Skin Surface Taken as 100
Unfiltered Radium Emanation Doses
Calculated for Y-Rays Only.
X-Rays- 30 cm Dist- 200 K.V.
½ mm Cu + ½ mm. Al. Filter
320 Ma. Min. Large Fields
74% - 3 cm
57.6% - 6 cm
Pack- 42,000 mc. Hrs- 15 cm Dist
2 mm Brass Filter.
Pack- 20,000 mc. Hrs- 10 cm Dist
2 mm. Brass Filter.
Pack- 10,000 mc. Hrs- 6 cm. Dist.
2 mm. Brass Filter.
Tray- 2200 mc. Hrs- 3 cm. Dist.
2 mm. Brass Filter.
Skin
3 cm.
10 mc. Buried Emanation Distributed at 8 Points.
Tumor Mass
3 cm. in Diameter
68.4% at Periphery.

Fig. 1. Diagrammatic representation of varying intensities of radium and *x*-ray doses. Note the marked superiority of buried emanation over all external applications.

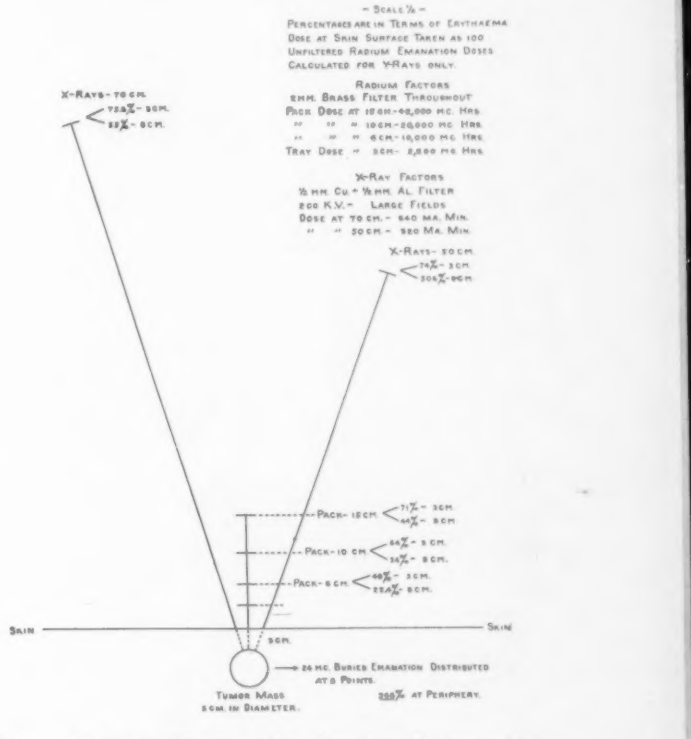
Scale ½
Percentages are in Terms of Erythaema Dose at Skin Surface Taken as 100
Unfiltered Radium Emanation Doses
Calculated for Y-Rays only.
Radium Factors
2 mm. Brass Filter Throughout
Pack Dose at 10 cm- 40,000 mc. Hrs.
" " " 10 cm- 20,000 mc. Hrs.
" " " 6 cm- 10,000 mc. Hrs.
Tray Dose " 30 cm- 2,200 mc Hrs.
X-Ray Factors
½ mm. Cu + ½ mm. Al. Filter
200 K.V.- Large Fields
Dose at 70 cm.- 640 ma. Min.
" " 50 cm.- 320 Ma. Min.
X-Rays- 70 cm.
72.8% - 30 cm.
55% - 8 cm.
X-Rays- 50 cm.
74% - 3 cm.
104% - 9 cm.
Pack- 18 cm.
71% - 3 cm.
44% - 8 cm.
Pack- 10 cm.
64% - 3 cm.
34% - 8 cm.
Pack- 6 cm.
48% - 3 cm.
25.4% - 8 cm.
Skin
Skin
3 cm.
26 mc. Buried Emanation Distributed at 8 Points.
Tumor Mass
5 cm. in Diameter.
200% at Periphery.

Fig. 2. Same as Figure 1, but showing relative values for a larger tumor.

retical viewpoint, at least, are incorrect in principle. Except in rare instances, it is impossible to get around the growth with a cautery, while placing of the heating agent centrally does not insure complete destruction within the limits of safety. Heat paralyzes the vessels at the periphery of its field of activity and hence tends to increase dissemination of the disease if any peripheral focus remains. The isolated reports of single or small groups of cases are not sufficient to warrant any practical conclusions. For the most part the same is true of reported results of surgical removal.

In 1909 Butlin[6] reported 9 cases occurring in his own practice and concluded that carcinoma of the floor of the mouth was not as dangerous to life as was generally supposed. He did not advocate any standard procedure, but applied the same excellent principles which prevailed throughout all his intraoral work. Of the 9 cases, 4 died of local recurrence, 1 of recurrence in the cervical nodes, and 4 were successful. Of these, 2, with movable lesions at the frenum of the tongue, had the cervical nodes removed shortly after removal of the primary growths and were well three years later, at the time of his report. One case was well ten years after without removal of nodes, and the other, twenty-two years after, without removal of nodes. It is obviously unfair to draw conclusions from such a small group, but it is interesting to note that his best results were in the two early cases without interference with the cervical lymphatics.

In the strictly surgical field, Morestin's work is the most extensive. He has advocated a standard procedure which he has

varied and amplified from time to time, always, however, maintaining the same principle—that of going as far from the disease and as near to the spark of life as possible. Briefly, his procedure was to split the lower lip, remove the upper half of the mandible on both sides as far back as the first molars, the mucosa and musculature of the floor of the mouth wide of the disease, a portion of the tongue and the cervical lymphatics. This left a good deal of healing to take place by granulation, but he counted on the remaining bridge of mandible to maintain the symmetry of the face and reconstructed mouth as far as possible. Later he divided his operation into two or three stages, removing the lymphatics first in one or two stages and finally removing the primary growth. While Morestin undoubtedly devoted a great deal of attention to this type of disease, he had but few cases to report. In his 1912 report he stated that to date he had seen only 11 cases in which his procedure was applicable. Of these, 3 never came to final operation, as they refused more than the cervical ablation or had unfortunate results after the preliminary work. Of the remaining 8, 1 died of bronchopneumonia following operation, 2 were lost track of, 1, free from cancer, died of pulmonary disease. Of the 4 cases followed, 1 had a recurrence after two years, 1 was well over four years, 1, nearly four years and 1, fourteen months.

In our own work we are opposed to surgical removal of the primary growth in this group of cases, for various reasons.

In the first place, only a small percentage of cases are, when they apply for treatment, amenable to a procedure even as extensive as Morestin's. Of these, only a part are able physically to stand the operation, even then running the risk of a high operative mortality. This leaves the great bulk of the cases on our hands in the very beginning. The mutilation of the operation itself, granting a favorable outcome, is enough to make both patient and surgeon shrink from it.

Aside from these practical factors, there are theoretical considerations. The trauma

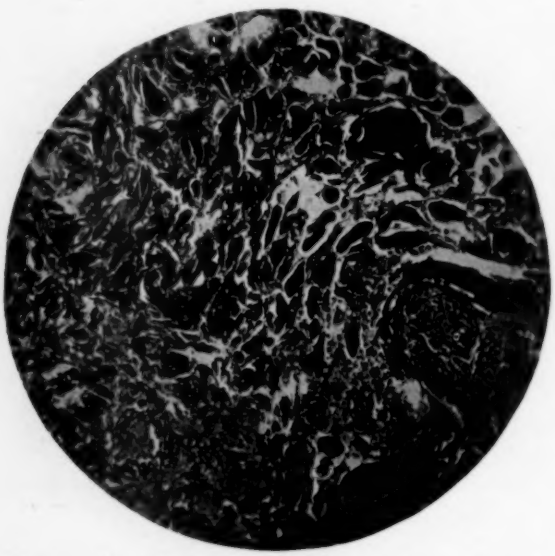

FIG. 3. Cervical node treated by maximum dosage of external radiation and removed one month later. Active tumor cells still present.

FIG. 4. Cervical node treated by buried radium emanation tubes and removed three weeks later. Complete destruction of tumor tissue.

incident to surgical removal in this location may very well disseminate more trouble than the knife removes. Any effort at local removal of a nonencapsulated growth, at least, only serves to destroy the defensive agents of the body—lymphocytes and plasma cells which are thrown out as a barrier round about the periphery of the growth. It has also been shown that loss of blood facilitates more rapid growth in any remaining focus of disease.

radium. We now feel, after over six years' experience, that this course has been amply justified.

In advocating the use of physical agents for the treatment of carcinoma of the floor of the mouth, mention must be made of *x*-rays as well as radium for their share in the work. In fact, I have been prompted to make this report at present for the purpose of calling attention to the lack of correlation of the two agents

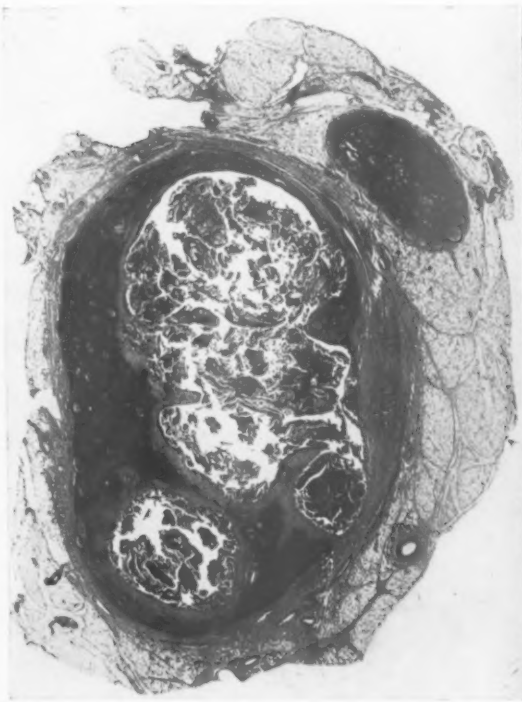

FIG. 5. Cervical node treated by buried radium emanation and removed one month later. Complete destruction of tumor tissue.

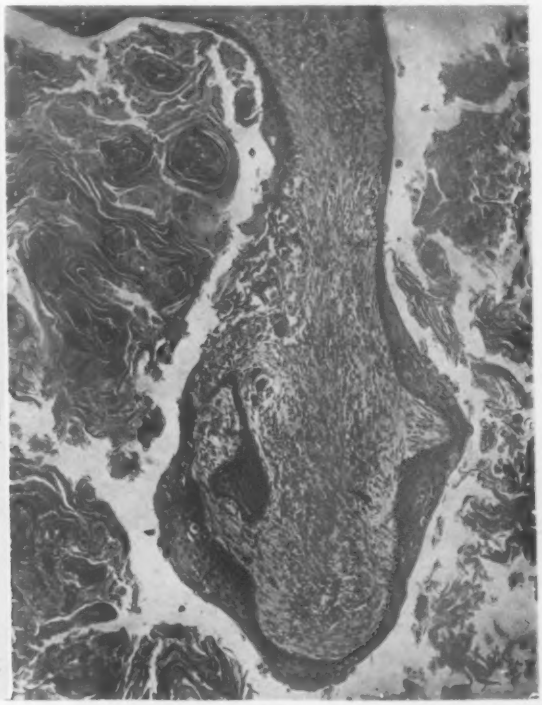

FIG. 6. Higher magnification of portion of capsule shown in Figure 5. Note the marked lymphocytic infiltration.

When we began using radium in intraoral cancer several years ago, favorable effects on tumor tissue were noted. When, in 1916, we began burying radium emanation in tumor tissue these favorable effects became much more marked. Palliative results not previously seen were obtained, and with improvement in technique of radium application went improvement in results. It was only reasonable to suppose that a method which would produce these changes in advanced cases would give much better results in earlier cases. Consequently we withdrew surgery from the primary intraoral lesions in favor of

in some quarters. In dealing with this disease, these two agents form an excellent combination, and while I have seen complete regression of the disease from radium alone, I cannot say the same of *x*-rays. While I believe that *x*-rays are quite equal to radium for the external application, I must also say that I have never seen complete regression of a primary carcinoma of the floor of the mouth, by external application alone, of either agent. Neither have I seen what I would consider an authentic report of such a case. The fibrosis produced by repeated doses, while tending to inhibit the growth of

tumor cells, also protects those surviving from subsequent applications of external radiation. There is a popular impression at present that the only requisite for complete regression of this type of disease is the uniform delivery throughout the tumor of a so-called "cancer dose." My own experience has been that it takes several times this amount of radiation to cause a clinical cure of epidermoid carcinoma.

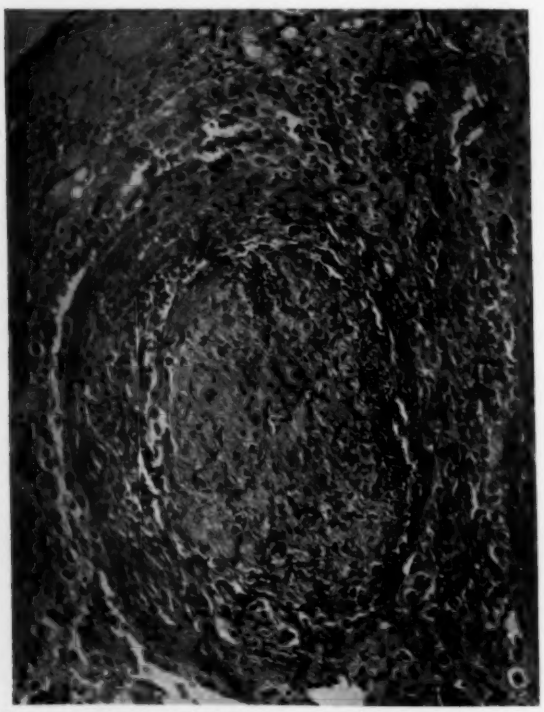

FIG. 7. Cervical node treated with buried radium emanation and removed two months later. Note fibrosis and lymphocytic infiltration.

In common with all intraoral carcinomas, the treatment of those arising in the floor of the mouth should be considered in two parts: First, treatment of the primary lesion; second, treatment of the cervical nodes.

In treating the primary growth we employ unfiltered tubes of radium emanation buried uniformly throughout the involved area, being especially careful to place them well to the limits of the palpable infiltration. A description of these unfiltered radium emanation tubes, their method of distribution, method of estimating dosage and advantages over interstitial use of needles containing radium element, can be found in previous publications.[7] These tubes should be placed so that there is, as nearly as possible, one tube per cubic centimeter of tumor tissue. This of course varies with the size and shape of the neoplasm. The preferable strength per tube is approximately one millicurie. Weaker tubes do not afford sufficient gamma radiation unless a larger number are used, and this produces too much trauma. Stronger tubes cause an excessive amount of necrosis. A study of the diagrammatic drawings in Figures 1 and 2 will show the relative values of the different forms of external radiation ordinarily employed, and their comparative relation to a safe dose of buried emanation. This estimate of buried emanation considers only gamma radiation and takes no note of the beta ray effect, probably one of the most important factors in interstitial radiation. It is also a very conservative calculation because the number of point sources is lower than that used in actual practice. It will be seen from these diagrams that the gamma ray effect alone from the dose of buried emanation is three to four times as great as the strongest cross-fire of heavily filtered radium at 10 cm. and 15 cm. distance or high voltage *x*-rays at 50 cm. and 70 cm. distance. These two latter are approximately the same in their efficiency. Smaller doses of heavily filtered radium at closer range are inferior to both. The tubes have not, in our experience, caused trouble as foreign bodies. There are, however, some disadvantages from their use in this location. Bathed as it is by mouth secretions in the most dependent portion of the oral cavity, infection is always present and the chronic inflammation adds to a radium inflammation which in itself is quite painful. As a result of this, the slough may be more extensive than with corresponding lesions in other locations. Wherever the slough tends to be deep the external carotid artery on the involved side, or on both sides if the lesion extends beyond the midline, should be ligated just above the superior thyroid branch. To reduce the anastomotic circulation

to a minimum the lingual and facial arteries should be tied separately. This procedure can be readily done under local anesthesia, and produces no undesirable after-effects. Whenever the neck is opened for this purpose, radium emanation should be buried in the lymphatic tissue at the upper and lower angles of the wound as well as about the posterior portion of the submaxillary lymphatics.

In burying emanation in the floor of the mouth, it is practically always necessary to place tubes near the mandible, and this frequently results in devitalizing a piece of the adjacent bone. The separation of this sequestrum is slow and often painful, but to date we have found no way of avoiding it or of facilitating its removal.

FIG. 8. Third stage of neck dissection. Note points of ligation of vessels.

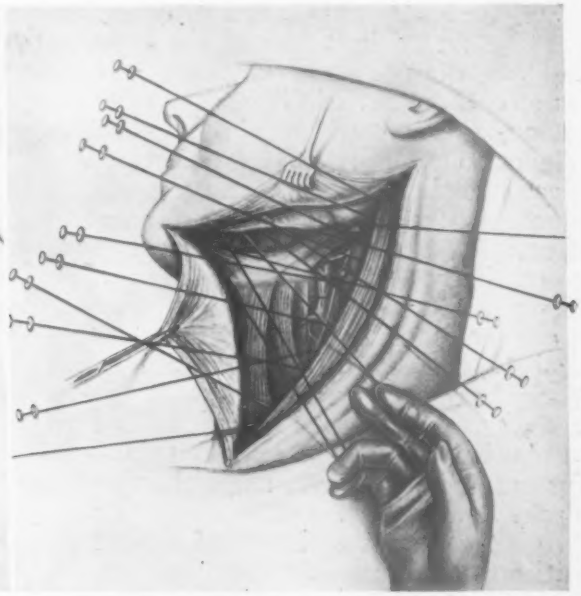

FIG. 9. Fourth stage of neck dissection. Note method of inserting unfiltered radium emanation tubes into base of surgical field.

In the treatment of the cervical lymphatics we favor a conservative procedure. Our views on this subject are stated in greater detail elsewhere, but may be briefly reviewed as follows:

We believe that the cervical lymph-nodes perform a conservative function in controlling and limiting dissemination of the disease, at least in the earlier part of its course. We also believe that metastatic extension of the disease is by embolism. The distribution of involved lymph-nodes and frequent single involvement of nodes secondary to intraoral carcinoma strongly support this view. Many of the patients applying for treatment are physically unable to undergo an extensive routine bilateral block dissection. In view of these considerations, we do not do a routine block dissection on each case as it comes to us. Instead, we treat each neck as a routine, with *x*-rays, for two reasons: First, to aid the lymph-nodes both directly and indirectly in the destruction of tumor cells and stimulation of the protective defenses of the body cells, thus combating secondary extension of the disease, and to produce at least a partial obliteration of lymph channels; second, to supplement the treatment already given to the primary growth. If at this time the neck is free from palpable involvement we do nothing more than use *x*-radiation or heavily filtered radium externally and keep the case under observation. If an invaded node is present, or appears under observation, we do a unilateral block dissection under local anesthesia and bury radium emanation at all suspicious points in the wound.

If a node or group of nodes in which the disease has already perforated the capsule of the node, is present, we expose the nodes, bury radium emanation uniformly throughout them and close the wound. The latter group is proving to be a very interesting study, and while there is not sufficient data at present to report on it, I believe it justifies the hope that we may ultimately replace all cervical lymphatic removal by interstitial radiation. Whenever the neck is exposed surgically, either for dissection or implantation of emanation tubes, advantage should be taken of the exposure to ligate the vessels if the primary lesion is not entirely healed. The same exposure often offers an added chance to bury emanation through the submaxillary space into the base of the primary growth. Emanation tubes may often be buried directly from without through the submaxillary region to the base of a deeply infiltrating primary growth, but not as accurately as with surgical exposure. Similarly in advanced cases with bulky cervical nodes where only very temporary palliation can be hoped for, the tubes may be buried directly through the skin. For accuracy, however, surgical exposure is usually necessary.

In all cases an estimate of the possibilities to be reasonably hoped for should be made before instituting treatment. If complete regression of disease can be anticipated, it is justifiable to use doses to the limit of tissue tolerance, even at the expense of considerable reaction on the part of the patient. If only palliative relief can be expected, then the comfort of the patient should hold first place throughout, and dosage and methods be modified accordingly.

In presenting our statistics of this group of cases for the past six years, we do so fully realizing their limitations. The cases were unselected and were treated as they came to us. All but a very few were far advanced and many of them would probably have been better off had treatment been confined to oral hygiene and opiates. The series can in no way be compared with surgical statistics from selected cases. Since technique has been gradually improved, we feel that better results are now obtainable than were possible in the beginning of the series. The period of observation in the majority of cases is too short to permit of absolute conclusions.

During the period from January 1, 1917 to December 31, 1922, we treated in all 113 cases of carcinoma of the floor of the mouth. These were divided as follows: Primary without palpable involvement of cervical nodes, 35 (of these, 10 cases subsequently developed infected nodes); primary with involvement of cervical nodes, 65; recurrent locally, 6; recurrent in cervical nodes only, 5; recurrent both locally and in cervical nodes, 2.

In attempting to classify these cases it may be well to explain that when we refer to palliative relief, we mean definite relief from symptoms for a time, at least, and prolongation of life over the natural course of the disease for a period long enough to make the procedure well worth while. Quite a number of cases listed as such are still living and enjoying reasonable comfort under the circumstances, a few up to two and three years. Cases referred to as unimproved are those in whom there was no relief from symptoms or alteration in the natural course of the disease sufficient to make treatment worth while. In referring to neck dissections we mean a complete unilateral dissection followed by implantation of radium emanation in the wound—in a few cases only a submaxillary or a sternomastoid dissection was employed. Surgical exposures for ligation of vessels or implantation of tubes only, are not listed as neck dissections.

The division by years is as follows:

In 1917, 4 cases; 2 were improved and 2 unimproved.

In 1918, six cases; 1 now free from clinical evidence of disease 52 months, 4 improved for a time and 1 unimproved.

In 1919, 16 cases; 4 are now clinically free from disease for periods of 44, 42, 39 and 34 months, 8 were improved and 4 unimproved.

In 1920, 21 cases; 9 are now clinically free from disease for periods of 34, 33, 31, 30, 30, 29, 27, 27, and 26 months, 8 were improved and 4 were unimproved.

In 1921, 17 cases; 6 are free from clinical

evidence of disease for periods of 20, 19, 16, 16, 12, and 12 months, 9 were improved and 2 unimproved.

In 1922, 34 cases; 4 are free from clinical evidence of disease for periods of 10, 8, 8 and 4 months, 11 have been improved, 5 were unimproved and 14 are too recent to classify.

Of 41 neck dissections done, 24 are now clinically free from evidence of disease, but this must be discounted because 10 were done in 1922, and several of the cases belong in the group of cases too recent to classify. Of the 30 done in the five years previous, 14 are now free from clinical evidence of disease. Only two cases put down in our records as clinically free from disease throughout have since recurred, one in 1920 and one in 1921.

Fifteen cases in the entire series showed secondary involvement of the mandible, and it is probable that this number would be higher had a routine *x*-ray examination of the lower jaw been made in every case.

Of the series, 37 cases are now known to be dead, 18 cases were totally unimproved, and 20 cases have been lost track of.

The net beneficial results from the total series of 113 cases to date are as follows:

Twenty-four cases are clinically free from disease for periods ranging, with one exception, from eight to fifty-two months; the average for the group being twenty-five and a half months.

Forty-three cases have been given palliative relief and of these 18 are still living.

Fourteen cases are too recent to classify.

CONCLUSIONS

1. Carcinoma of the floor of the mouth is a distinct clinical entity with peculiar therapeutic problems which render it unlike any of the other intraoral groups.

2. We believe our experience to date warrants us in advising interstitial radiation by means of unfiltered radium emanation tubes as the agent of choice in the treatment of the primary lesion.

3. We believe that the problem of dealing with the metastatic extension of disease to cervical nodes is best handled on a conservative basis, using a combination of surgery, radium and *x*-rays.

4. We make these tentative conclusions fully recognizing our limitations of observation period, number and type of cases treated.

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DISCUSSION

Dr. Pfahler. I am sure we all appreciate this very excellent paper of Dr. Quick, who has presented to us so carefully an analysis of his results.

I would like to emphasize the importance of cleaning out the mouth at the very beginning of the examination, getting rid of infected teeth, irritating plates, etc.

I have had no experience in the use of the emanation needles; therefore, I cannot discuss it from that standpoint at all. The results shown are certainly very convincing.

It has been the opinion of Dr. Widman and myself that we obtain our best results by the surface application, externally, of the roentgen ray, and now, of course, we are using higher voltage rays. Mr. Failla has worked out very nicely the depth value of this surface radiation. Most of these tumors are not probably 3 cm. in thickness, and therefore we will deliver a greater quantity to the tissue involved. It has been our custom, therefore, to apply roentgen rays externally to the limit of skin toleration and to apply radium by application over the surface of the tongue and floor of the mouth inside, and then after about a month, after we have had considerable fibrous tissue

formation, to imbed radium needles into the tumor tissue, very much on the same basis as that on which Dr. Quick uses the emanation needle. We have used element needles (10 mg. needles) and used one needle for each c.c. of tissue involved and leaving the needles in place for a period of six to ten hours, depending also upon the size. We have this one advantage in the use of radium element needles over emanation needles: that we are using only the hardest beta rays and gamma rays, and we are getting distant radiation effect to a considerable degree. On the other hand, we have not as many needles to distribute and we cannot get as wide a distribution of radiation effects as you get with emanation. The emanation needles, however, produce more local necrosis and therefore you have liquefaction processes, which we avoid.

I simply mention this application to those who do not have emanation needles to work with. We only have 500 mg. of radium to work with and therefore have not thought it advisable to reduce it to emanation.

Dr. Ewing. In regard to the use of these highly destructive agents in cancer, it seems to me that the ultimate position which they will occupy in comparison with surgery depends not so much on any particular skill or technique but more upon the principles on which they are employed. I have encouraged my clinical colleagues to proceed with the use of radiation on the ground that the methods were biologically sound, and especially with the local use of radium emanation needles in the treatment of infiltrating squamous cancer.

There is an impressive parallel between the action excited by radiation properly applied and that which nature produces against the disease. If those observations are correct, then you may say that radiation therapy is secundum naturam, in accordance with nature's methods. Therefore, we can say radiation is to some extent a rational therapy. I am under the impression that the ultimate position of radiation therapy will depend more upon the principles upon which we are proceeding than upon development of technique. We are dealing with the disease on a more rational basis than has ever been employed in the treatment of cancer.

Is there such a thing as immunity against cancer? From experimental data we have certain grounds for assuming that there is actual immunity against the progress of certain malignant neoplasms. Experimental conditions under which supposed immunity has been observed are that after regression of a tumor in the animal he becomes more resistant to implantation of the same tumor—at times completely resistant.

Does regression of cancer under radiation in the body, with absorption of the regressing cells, increase immunity? I do not think we have any definite clinical observations to justify such an assumption but we do have sound theoretical data to encourage us to believe that there may be some increased immunity. Our information is far from sufficient to enable us to draw positive conclusions on this matter. This general immunity reaction, which we may assume to exist, is an element entirely lacking in any other method of treatment of cancer, especially surgery.

Dr. Quick (closing discussion). I have nothing more to say except to reinforce one point which Dr. Pfahler made; that of heavy surface radiation prior to the imbedding of either radium tubes or needles. We try to carry this out in all cases and feel justified in doing so because it does cut down the danger.

Another point in the difference between burying emanation and radium needles relative to the beta ray effect: I am not in a position to say that it is an advantage to have beta radiation present. We do know that it produces a very sharp local effect. In my own experience I think it has been unusual to note that those cases with the most permanent result have been the ones associated with beta as well as gamma radiation.

None of the agents today form the ideal cancer cure, so-called. Consequently I think it is our business to correlate them as far as possible, to take care of the local problem, and still keep looking for the various ways to take care of the real problem, that is, dealing with the thing constitutionally.

INTRATHORACIC CHANGES FOLLOWING ROENTGEN TREATMENT OF BREAST CARCINOMA*

BY T. A. GROOVER, M.D., A. C. CHRISTIE, M.D., AND E. A. MERRITT, M.D.

WASHINGTON, D. C.

AT the midwinter meeting of the Eastern Section of this Society, held in January, 1922, one of us (Dr. Groover) in the course of a discussion on roentgen therapy of malignant disease, reported certain intrathoracic changes that we had observed, following treatment of breast carcinoma.

These changes were also referred to by us in a paper read in the Section on Radiology, Southern Medical Association, Nov. 14–17, 1921, and published in the *Southern Medical Journal*, June, 1922, Vol. xv, No. 6.

We had an impression at that time that some of these changes were due to the treatment instead of to extension of the original disease. This impression has been strengthened by observation of other cases and the further course of those then under treatment.

This communication is presented only as a preliminary report, and to stimulate discussion. We are convinced, however, that not all the changes observed in the chest after massive dose treatment of carcinoma of the breast are due to malignant disease: that at least some of them are attributable directly to the effect of the treatment.

Our attention was first called to this by the fact that patients given prolonged treatment through a copper filter behaved quite differently from those treated by the older method through 5 or 6 mm. of aluminum over numerous portals of entry. The skin reaction after treatment through copper filter is marked at ten days and reaches its maximum at about eighteen to twenty; it then gradually subsides with a fine branny desquamation and is complete in from four to six weeks. About the time that the skin reaction is distinct, the patient begins to complain of a dry hacking cough, and in some cases, of difficulty in swallowing. The latter disappears in about a week or ten days, but the cough persists, and when the skin reaction has reached its height, the cough is very hard, dry and unproductive and causes the patient much distress. During the time of subsidence of the skin reaction the cough gradually lessens in severity and in six to eight weeks disappears entirely. The fact that the symptoms of intrathoracic irritation follow so closely the course of the skin reaction seems very significant. Their coincidence in a considerable number of cases has led us to the belief that the delivery of a large dose to the deep structures by means of prolonged treatment through a copper filter has brought about changes in the pleura and even in the lungs, analogous to the changes that take place in the skin. This belief seems to us thoroughly rational, especially in the light of investigations that have shown that roentgen treatment over the abdomen is capable of causing definite changes in the mucous membrane of the small intestine, accompanied by diarrhea and bloody stools. It is also known that a dose of roentgen rays sufficient to cure at one application an epithelioma on the cheek will often cause a very marked change in the mucous membrane of the mouth. The mucosa corresponding to the area treated on the outside has exactly the appearance it would have had if pure carbolic acid had been applied. The superficial mucosa is destroyed and sloughs off, leaving a normal-looking mucous membrane, these changes taking place coincidently with the destructive and healing process about the lesion on the outside. Similar changes are known to take place in the larynx, pharynx and esophagus after roentgen treatment about the neck. We have repeatedly observed extensive reaction over the mucous membrane of the fauces, soft palate, and pharynx after prolonged treatment for malignant disease of the face, mouth, or neck, the radiation being directed entirely from outside. It seems reasonable to believe that analogous changes may occur in the epithelial lining of the bronchi, the lining membrane of the pleural sac, or even in the parenchyma of the lungs.

*Read at the Twenty-third Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Los Angeles, Calif., Sept. 12–16, 1922.

Our observations are based entirely on clinical and roentgen findings and are not offərəd as definite proof that the phenomena are due to the effects of the roentgen ray. There is no way to demonstrate from our patients what histological changes are present, except as we infer from the roentgen appearance. Proof that the treatment instead of the original disease is responsible for the change, could be had if we could subject patients not suffering from carcinoma to exactly the same treatment. The procedure is of too severe a nature to permit of this, but it may be mentioned that we have treated 3 patients with sarcoma of the thorax, one of the 7th rib, one of the clavicle, and one of the soft tissues in the scapular region, and that all of them have had the cough described above when their reaction was at its height. In one there were distinct intrathoracic changes at the right base similar to those seen in the accompanying illustrations.

We have been unable to carry out experimental work on animals, but we hope that this report will induce some one, properly equipped, to do it. We are aware of the work done by Warren and Whipple in the course of their studies of "Roentgen-ray Intoxication" which showed that the dosage used by them produced no demonstrable intrathoracic changes in dogs, while it did cause marked and extensive changes in the epithelium of the small intestine. The dosage used by them, however, by no means corresponds to that employed by us in treating breast carcinoma. Their dogs were treated through 2 mm. of aluminum, and the maximum dosage given was that represented by 512 ma. min., through comparatively small skin areas. Our total dosage directed toward the intrathoracic structures is represented by 2700 ma. min., and additional effect may be expected from the secondary radiation due to treating over large areas. It is only in the patients who have received these large doses that we have observed the symptoms and roentgen findings reported here.

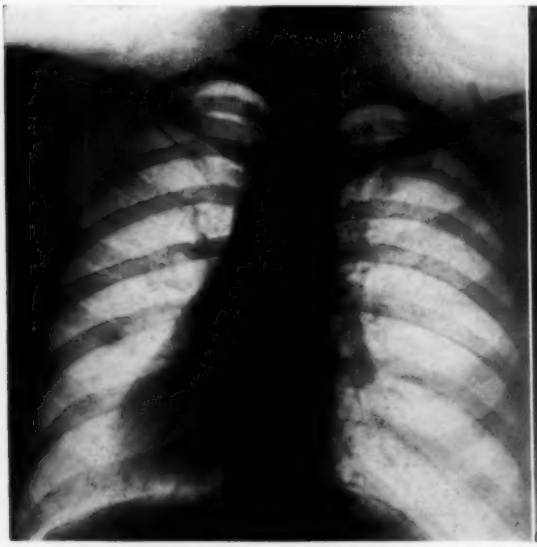

FIG. 1. Case I. May 4, 1921. Lungs are apparently normal.

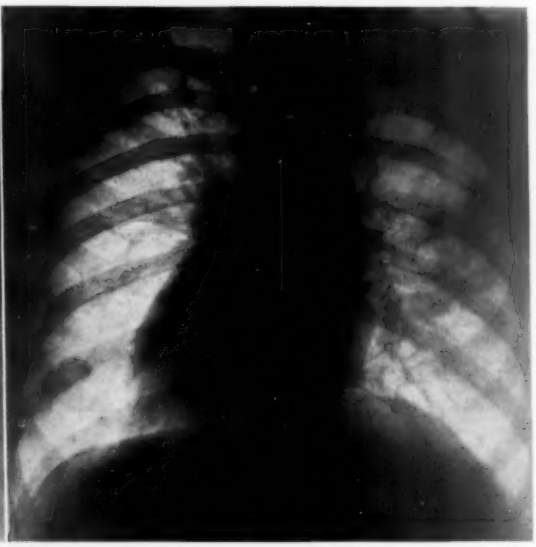

FIG. 2. Case I. July 7, 1921. Area of infiltration in right lung which was interpreted as a direct extension of the recurrent carcinoma of the breast.

The following case history, with reproductions of roentgenograms made at various times, illustrates the point we wish to bring out:

Female, unmarried, aged thirty-five, came under observation on April 30, 1921. Right breast removed June 3, 1920. A red spot persisted in the lower part of the scar, which, in a few months, developed into a rapidly growing tumor. It was conical in shape, about the size of a hen egg, very vascular, and projected outward

from the chest wall. It was apparently near to ulceration. There was a definite palpable mass in the right side of the neck just above the clavicle. Roentgen treatment was given through ½ mm. copper and 1 mm. aluminum filter with a spark-gap of 9 to 10 in. and 5 ma. of current. The treatment was as follows:

April 30, 1921. The right pectoral region and axilla, including the area of the tumor, one and a half hours at 12 in. distance.

May 3. The same area as above for the same time.

FIG. 3. Case I. July 29, 1921. Marked increase in extent of the infiltrated area.

FIG. 4. Case I. August 19, 1921. The roentgen appearance not essentially different from that of Figure 3 except that there is beginning fibrosis.

May 4. The right side of the neck and supraclavicular region, two hours at 10 in. distance. A roentgenogram made on this date is shown in Figure 1.

May 6. Right scapular region, one hour at 12 in. distance.

May 7. Right scapular region, forty minutes at 12 in. distance.

May 10. Right scapular region one hour, twenty minutes, at 12 in. distance.

The patient thus had from April 30 to May 10 a total of three hours over the right pectoral region and axilla, three hours over the right scapular region at 12 in. distance and two hours over the right side of the neck at 10 in. distance.

May 28. Marked erythema over chest and neck.

June 9. Skin reaction subsiding. There is definite reduction in the size of the tumor and in the mass in the neck.

July 7. The tumor on the chest wall has almost disappeared. The patient complains of cough, which she has had for about three weeks. Roentgen examination of the chest on this date shows a definite area of infiltration in the right lung. The appearance suggests direct extension from the tumor on the thoracic wall, rather than metastasis (Fig. 2).

July 29. The external evidence of disease has practically disappeared. The general appearance is good, but there is considerable cough and shortness of breath on exertion. There is marked increase in the process in the right lung except that there is evidence of beginning fibrosis (Fig. 3).

August 19. No evidence whatever of disease externally. The expansion of the right chest is much restricted. The roentgen appearance is not essentially different from that of July 29 (Fig. 4).

September 13. The cough has almost disappeared. Still some embarrassment of respiration on exertion. There appears to be distinct retrogression in the process in the lung. There is evidence of fibrosis and there are adhesions at the base (Fig. 5).

September 29. The expansion of the right chest is restricted, the right diaphragm elevated and immobile and there has been gradual retraction of the heart and mediastinal structures toward the right. There appears to be distinct fibrosis in the right lung (Fig. 6).

The patient was lost to observation until May 23, 1922, when she reported again with an extensive local recurrence in the same location where the tumor was before. She states that this began to appear in February, 1922. There is now a large cauliflower mass with bad odor. Examination of the chest shows a much retracted lung field on the right side, but the lung aerates well and there is almost complete disappearance of the markings previously interpreted as representing fibrosis. There is no evidence of the presence of intrathoracic disease (Fig. 7).

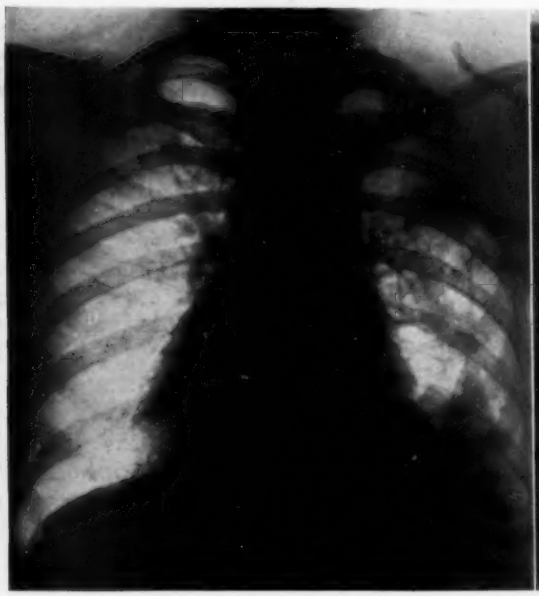

FIG. 5. Case I. September 13, 1921. The evidence of infiltration has largely disappeared. There are marked fibrosis throughout the lung and adhesions at the right base.

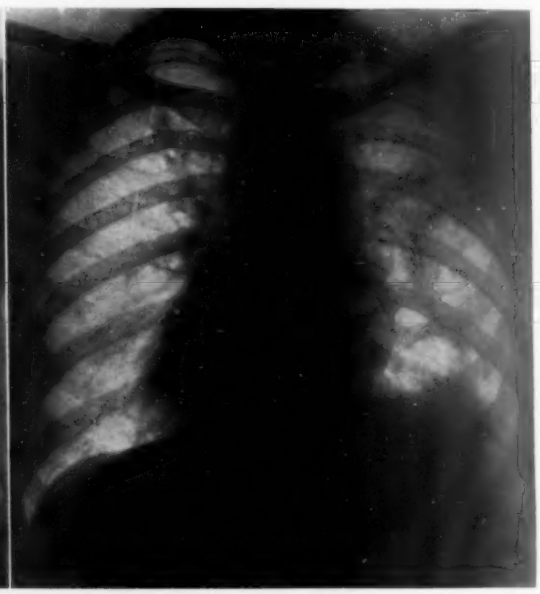

FIG. 6. Case I. September 29, 1921. Retraction of mediastinal structures toward the right. Right diaphragm higher than normal, and immobile.

On May 31 the tumor was removed by electrocoagulation and the area of the tumor given radium and roentgen treatment.

The points we wish especially to emphasize in this case are that this patient had a lesion in the right lung which appeared after roentgen treatment and was accompanied by cough and dyspnea; that this was presumably due to extension of the carcinoma of the breast, but that it subsequently disappeared; and that the patient is free today, after sixteen months, of any demonstrable intrathoracic disease.

Not all of our cases have been so clear-cut as this one. In some of them we are sure that there has been extension of the malignant disease to the intrathoracic structures. But even in these cases, the occurrence and subsequent disappearance of cough and dyspnea coincident with a partial clearing up of the evidence of infiltration in the lung have strengthened our conviction that roentgen treatment given in the way described does actually produce changes in the pleura and lungs.

DISCUSSION

DR. TYLER. The lung changes following heavy radiation therapy have been noted by me for a considerable period of time. I agree with Dr. Christie in his description of the findings shown on the *x*-ray plate of the chest. In some of these cases the changes take place in the lung immediately following treatment. The changes can be demonstrated both roentgenologically and by physical examination, all of our cases having been checked up by all known diagnostic methods. If the changes

come on acutely following the treatment, and treatment is stopped, the lung will gradually restore itself to nearly normal. There will, however, be left in the lung at the site of the lesion, scar tissue which is permanent and is demonstrable on the *x*-ray plate during the entire life of the patient.

Another type of case comes on very insidiously. The first symptom described by the patient will be shortness of breath. In this type of case the process is progressive, the scar tissue gradually increasing in the lung, and in some cases we have even had retraction of the lung from the chest wall without the formation of fluid in the pleural cavity. In others, we have had pleural effusion showing with contraction of the lung. In those cases the fluid, when drawn off, is straw-colored and is not bloody.

So far, we have been unable to get post-mortem examination on any of these cases, although we have recently had two deaths in this type of case.

We note with considerable interest that Dr. Case calls attention to these changes in a paper read at the last meeting of the American Medical Association and published in the *Journal of the American Medical Association* for August 26, 1922, page 699. In the same number of the *Journal of the American Medical Association*, page 720, is an article by Dr. Laurence E. Hines of Chicago written under the title, "Fibrosis of the Lung Following Roentgen Therapy Treatment for Tumor." It has been his happy experience to get post-mortem examination on two such cases, and he describes very interestingly the microscopic findings in the lungs of these patients which bear out the suggestions offered by Dr. Christie. In both of these cases reported by Dr. Hines, carcinoma was present in one and sarcoma in the other. Fibrosis shows, however, in both, with thickening of the walls of the bronchi, resulting in complete obliteration of the smaller ones as well as the terminal alveoli of the lung.

Dr. Stewart. It might be interesting for you to know that I have here a paper from Dr. H. Wintz, who probably has the most accurate roentgenological clinic in Germany. He states:

"By far more dangerous is the infiltration of the pulmonary tissue which may occur after systematic irradiations of mammary carcinomata or after raying of tumors located in the lung. The result of the examination of such a lung resembles that of a central pneumonia. There is no fever, but a slight irritative cough. For the respiration the whole affected part of the lung is, of course, useless. The condition is also comparatively harmless, and will recede spontaneously in the course of one-half to three-fourth years. But the concurrence of a disease (pneumonia bronchitis) would render the prognosis unfavorable."

Dr. Cathcart. Dr. Christie is to be congratulated for bringing to us at this time warning in regard to effects we may expect from deep roentgen-ray therapy.

I wish to report on two patients who, following breast amputation for carcinoma had local and cervical recurrences and were given deep *x*-ray treatment (9½ in. gap) to the point of skin tolerance over a period of about one year.

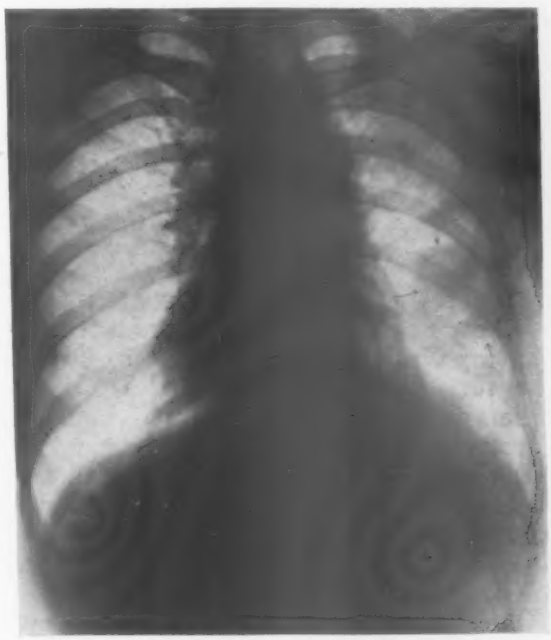

Fig. 7. Case I. May 23, 1922. Some fibrosis in upper part of right lung. Diaphragm in normal position and freely movable with respiration. The shadow indicated by the arrows is not due to intrathoracic changes, but to the recurrent tumor on the chest wall.

Each developed a very irritating cough. Skiagraphs showed heavy fibrosis radiating from the hilus. This was thought to be malignant and treatment was discontinued.

It is now one year since the last treatment. The cough has disappeared and radiographs show a marked decrease in the bronchial shadows.

Dr. Bissell. I wish to say that I have observed the same thing in three cases, and I followed them rather closely. They were all under observation at the time Dr. Tyler read his paper, and subsequent to that I followed them, and I want to say that it is not safe to speak of it as fibrosis. There is fibrosis left after the inflammatory reaction leaves. It

acts like an inflammatory process, rather acute in its onset, and the patients have all complained of very severe compression of the chest.

I would like to ask of Dr. Christie: while these cases were radiated throughout the chest, in every instance the reaction was all on the one side. Why should it occur on one and not on the other? In my cases the reaction is always on the affected side.

DR. CHRISTIE (closing discussion). Dr. Bissell suggests that we ought to emphasize the fact that it is not always possible, in any given case, to tell whether we actually have a metastasis in the chest or whether it is this infiltrative change due to treatment. Of course, it is not always possible, and as I said, not all of our cases have been so clear-cut as this one, because in some there were undoubtedly malignant changes.

I believe that the change is actually of non-infective inflammatory character, and that its final state is of fibrosis, that in the beginning it is infiltrative and actually an inflammatory change in the lung structure.

We believe that the coincidence of all these symptoms with the treatment proves quite definitely that the treatment itself is responsible for many of the changes we have observed.

A REVIEW OF THE PRESENT STATUS OF DEEP ROENTGEN THERAPY*

BY GUIDO HOLZNECHT, M.D.

Professor of Medical Roentgenology

VIENNA, AUSTRIA

SINCE Freund made his first attempt at the therapeutic employment of the roentgen rays, since Kienboeck first suggested the fundamental principles of roentgenotherapy, since the author (Holznecht) constructed the first dosimetric apparatus (1901), since Senn, of Chicago, with the roentgen treatment of leukemia, inaugurated the era of deep radiation, the problem of dosage has remained the center of our interest.

The question of dosage has not everywhere passed through a uniform development. In Germany it presents an aspect differing from that in America, France and England. The School of Vienna accepts some of the German results, while rejecting others and combining the experience gained in the past with modern knowledge.

The arrangement and standardization of dosage has to overcome two difficulties, namely, the physical and biological, both of which continue to receive new accretions through the ever-increasing knowledge regarding the physical and biological activities of the rays in the body, and, at last, through the overwhelming significance of scattering ("Streuung"). As regards the physical dosage, it seems that in principle it approaches a certain finality, which is of the utmost importance for the question of the biological dosage. As long as we are unable to weigh and measure the remedy, we shall be unable to get an exact knowledge of its biological effect.

We can study the status of both problems by directing our attention to the conceptions formed regarding them, which like land-marks indicate the road over which the investigation has traveled. Regarding the physical problem, we may follow Volz, whose conclusions, again, are based on the findings of other authors. With Volz we differentiate two general and three special dosimetric conceptions. Dealing first with the "physical dose" in the stricter sense of the term, we see that it does not take any cognizance of the bodily relations, nor of any manifestations exhibited during the passage of the rays through the body, and considers only the density and penetrative power of the rays by measuring and registering them.

Then the quotient of dosage, the proportion between the incident radiation and the radiation present at any depth: This conception, omitting nothing, comprises all effective factors, the kind of primary radiation, the focus-skin distance with its well-known significance, the portal of entry with its scattering effect and the qualities of the body. For practical pur-

* Read at the Twenty-third Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Los Angeles, Calif., Sept. 12–16, 1922.

poses it seemed to be advisable to formulate two additional special dosologic conceptions. For the limitless variety of the respective factors necessitates the establishment upon concrete premises. The special conception of the "percentual deep dosage" is based on the focus-skin distance of 23 cm. at a depth of 10 cm. and a portal of entry of 6 to 8 cm. Leaving out of consideration the size of the portal of entry, the conception of the "effective dose" becomes amplified. Moreover, by leaving the focus-skin distance and the depth without a special determination, the result will be a measure which has been designated as "utility dose." The latter changes from place to place on the irradiated body, which we may imagine as being filled by the numbers of the doses. This picture will be endowed with system and order if we conceive of the points with equal doses as connected by lines. You are acquainted with these pictures of transverse sections which contain such a multiplicity of lines. These appear as the most interesting mohair-like designs, if we think of the body as irradiated not from one direction but from different sides, because then several additional dosage numbers become assigned to each point. Only by proceeding in this manner shall we be enabled to obtain a full insight into the distribution of the rays in the body. But we must not think of these doses as being in accordance with our old posologic conceptions—namely, quantities, but as intensities which are effective at any instant or in any unit of time. If these are summed up during the course of a radiation we obtain the "surface energy" and its distribution, and herewith an exact idea of the quantity of roentgen rays in the body. Simple as these results may appear, they represent a superabundance of work.

We were not as successful with the investigation of the biologic dosage and its formulation. The conceptions "destructive dose," "paralyzing dose," "stimulating dose," "skin unit dose," "carcinoma dose," "ovarian dose," " tuberculosis dose," etc., have all been found wanting for practical purposes. Evidently, under the influence of a mechanistic natural philosophy, there has taken place what might be called a standardization of pathology which years ago met with scant favor. They all are of significance as working hypotheses, from which the investigation proceeds and advances, but for practical purposes none of them seem to be available, especially not the conception of the "effect of stimulation." Its value has been assured for the semen and the ova of lower and higher organisms, but it has not been proved for new growths. In spite of many thousand cases of cancer treated during the last twenty years with weak radiations, the numerous respective assertions are supported by only a few more closely observed cases of an apparent promotion of growth, and even if we were willing to accept them, they represent but exceptions from which no rules can be formulated. Moreover, it is quite improbable that the rays would exercise a purely depressive or purely promoting influence upon the living cell. Yet, it seems to be probable that there arise various "alienations" which, as a matter of course, may contain depressive or stimulative components. Science will continue to explore them, while for practical purposes we would better remain on the terra firma of facts and experience. How far one may go astray was made manifest when —from an over-estimation of the defensive forces of the organism, especially the connective tissue, against carcinoma—small doses were recommended and thus the precious acquisitions of our experience were jeopardized. For the treatment of carcinoma the high doses are to be continued. As to the exact amount of the doses, we have learned valuable lessons from the treatment of sarcoma. On the proper dosage in sarcoma, the statistics collected by Juengling (1905, 1912, 1914, 1920) have thrown light: 30 per cent of the sarcomata disappeared primarily; 50 per cent shrunk; 20 per cent remained absolutely refractory, no matter whether several millimeters of aluminum or brass and zinc were employed with the old or modern technique. Even without filtration, there were still 17 per cent of primary disappearance. We do not speak of a cure, but merely of primary disappearance, which is selected as an estimation of the effects of the different methods. All this goes to show that the

	Filter	Disappearance, Per Cent	Shrinking, Per Cent	Uninfluenced, Per Cent	Total Cases
Kienboeck, 1902–1905	None	17.8	57.8	24.4	90
Chr. Müller, 1912	Aluminum	31.4	45.7	22.8	35
Seitz Wintz	Heavy filter thin	31.4	48.6	20.0	70
Clinic Tübingen	Heavy filter thick	31.9	44.7	23.4	47

improvement in the technique above a rather low degree was of no avail. It is not we, but the particular form of tumor which determines whether our treatment will be successful or not. To remember this is important. It would not be right to torment the patient by exposing him to secondary effects, if we are unable to conquer the sarcoma by this method. I believe, maybe with some of you, that in the case of a carcinoma we are confronted by the same conditions, although of a higher technical degree. As sarcomata of the lymphatic glands are distinguished by their sensibility to a low technique, among the carcinomata those of the uterus are responsive to a higher technique. Of microscopically well-characterized sarcomata some disappeared primarily, while others remained refractory. The same observation has been made with the carcinomata. In spite of the most phenomenal results obtained in a few cases, the fact remains that all the technical improvements in the world will not enable us to triumph over the carcinomata absolutely. It is dignified to acknowledge our inability and to confess that we are confronted by a biological barrier over which to leap is not in our power. There remains nothing to be done but to find the still unknown technical degree for the carcinoma, which may possibly be accomplished by means of the modern therapeutic apparatus. This has always been our opinion regarding the other, especially the internal diseases which, owing to the interest devoted to the carcinoma, have been too much neglected. The standardization and determination of the dosage is difficult but important, for the practice requires normalization. If we form theories from our experience with the various affections, and write down the best technical measures, we shall find ourselves confronted by a chaos of various posologic prescriptions, if I am allowed to use this expression also in our department. They must, of course, contain technical factors, because the dosage is dependent upon them and even upon the intervals between the series. In order to simplify our task, the manifold affections and prescriptions must be first formed into several groups. And indeed, we can easily arrange the different affections in four groups, namely, those that require an extremely large quantity (where in solitude the carcinoma is enthroned); the sarcomatous and certain other affections requiring a considerable quantity; those requiring a medium quantity; those requiring a small quantity. If we want to prescribe uniform technical formulae for each group of diseases we shall find ourselves hampered by the differences in the size of the parts of the body and the varying depth of the tissue which call for different fields and different quantities. These difficulties may be overcome if, in place of the different sizes and forms of the parts of the body which are to be irradiated, we substitute a limited number of definite variations in their size. I propose five degrees of sizes, namely, parts of the body of approximately 5, 10, 15, 20, 25–30 cm. thickness. To these are to be adjoined, for each group of diseases, the corresponding number and size of the fields. The formula remains the same for each group. As a matter of course, some diseases require exemption from the general group formula. The hardness of the rays—which hitherto was measured by the spark gap or the number of kilovolts in the apparatus (both quite inexact) or in the absorption experiment with the "selenium cell" (simple but not exact); or with the iontometer (very cumbersome)—is now to be determined by the shortest wave-length and by the wave-length of the maximum of radiation. Both may easily and exactly be determined by means of the new spectrometer (March, Staunig and Fritz of Innsbruck). In employing the latter, we do without the photographic plate and read from a fluoroscopic screen. Our tabulation

gives the hardness of the rays as well as indications of the spark-gap, the main data of the radiation mixture, the shortest wave-length and the maximum of radiation in "Ångström units."

I beg to submit to your judgment my proposition which has become feasible by the fact that during these latter years roentgenotherapy has made immense advances.

PRACTICAL ROENTGEN-SPECTROMETRY AND ITS PHYSICAL BASIS*

BY PRIVAT-DOZENT DR. K. STAUNIG

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THE "qualimetry" of roentgen rays (determining the hardness, power of penetration, lengths of waves) must, as is easy to see, form the basis for every practical use of the rays in general and for the dosimetry of the roentgen therapy in particular. For, as it is impossible to recognize the value of large sums without knowing the value of the unit coin of this standard of currency, or, as the artillerist who does not know the range of his cannon, cannot reckon on the exact effect of his guns and runs the danger of damaging his own lines, so in the practical use of roentgen rays the knowledge of the power of penetration is a preliminary condition for the exact comprehension of any effect which the rays should produce.

We know, for instance, by experience, that each radiogram needs special conditions of exposure, according to the thickness and density of the object to be reproduced. Under these conditions the *quality* of the radiation is of decisive importance. What *quantity* of radiation is needed for a radiogram is of secondary importance. In the same way the roentgen penetration also needs a qualitative definition of the usable rays. Although empiricism has abolished grave mistakes and errors here, the choice of the suitable radiation, the reliable filtering and the statement of the appropriate quantity of this radiation for every substratum to be penetrated, will only be possible when the problem of qualitative analysis has found its solution. A burning question, however, is that regarding roentgen therapy. The rays have proved a medicine which has general and local effects. To dose this medicine means to give it in qualitatively and quantitatively fixed amounts. As long as the quality is not exactly ascertained, it is like a solution whose composition is not or is only inaccurately known. Nobody would care to prescribe such a solution, even if he had the means to dose it quantitatively, as, for instance, in c.c. Empiricism, it is true, by using rule of thumb methods, has formed a system which enables us to avoid injuries, and has been of great value; but this does not alter the fact that the real problem is still awaiting its solution, and that the unravelling must start from the point of the qualitative analysis of the radiation.

None of the methods used up to now led to the exact physical basis, that is to say, to the *knowledge of the wave-lengths of the radiation;* they had to be content with an insufficient approximation. So, for instance, the new method of filtering analysis has declared radiations as homogeneous, which really contained differences of a whole octave of wave-lengths, as was proved by Seemann's spectrometric experiments. The exact examination of measuring methods by the special committee of the German Roentgen Society has brought to light a whole series of sources of errors and defects, so that today we consider the whole structure of qualimetry as shaken, and must think of a complete new construction of the dosimetry.

A new system could be built up only with support of the method of ray-analysis, made use of by physics; especially as we had to deal with methods which had already brought unforeseen success in

*All researches on this subject have been carried on with the help of the Rockefeller Fund. Detailed literature in *Fortschr. a. d. Geb. d. Röntgenstrahlen.*

broad spheres of science. Clear and distinct as the results of the physical experiments were, especially as to exploration of the roentgen spectrum, still they have not attained that importance for practical radiology which they ought to have; partly because there was not enough attention paid to them, partly because the methods were really not adaptable for practice on account of their unhandiness and intricacy. It took up hours of exposing time, for instance, to project a spectrum; choosing an adaptable crystal and adjusting it needed such accuracy that it seemed to exceed the limit of what could be expected of a practicing physician. The physicist, however, was led by his own interests into exceedingly fertile regions, but regions which were far from the sphere of interest of the practicing radiologist. So the physicist went on his own way in ignorance of the needs of practice; the practitioner, on the other hand, overlooked or underestimated the importance of physical researches.

When, on this account, we, March, Fritz and the author, in the spring of 1920 started in Innsbruck to establish relations between the physics and the practice of radiology, the problems could be seen in distinct form. There could be no doubt that the crystal analysis had to form the basis of a spectral-analytic process and that the first problem was to find a practical form for it. It ought to make possible a direct insight into the roentgen spectrum; it ought to adapt itself to the conditions of radiological practice and its simple resources. It should be possible to carry it out in a short time and to repeat it as often as one wants to. It should be accurate enough, in spite of its simplicity, to make possible reasonably exact measurements of the wave-lengths. The second problem was the following: A single quantity which should be able to characterize with sufficient accuracy the polychrome complex of the radiations, had to be found for every spectrum, corresponding to a radiation. In other words, a key had to be obtained which could serve in practice as a simple term for each radiation. The third problem was the utilization of this key in practice. Before we proceed to give a short explanation of our spectrometric method of analysis, and to describe our spectrometer and its utility, we will give a short résumé of the results of the physical experiments, as they appeared to us at the beginning of our researches or were supplemented during the course of our work. This seems to be especially necessary, as these important results, though not unknown in radiological circles, were at least greatly underestimated as to their importance for radiology in general.

PHYSICAL BASIS

The spectrometric method of examination found its introduction in Laue's renowned experiment. In this, a bundle of primary rays penetrates a fixed crystal; the rays are reflected by numerous planes which are regularly studded with atoms. Each reflection takes place at a host of these planes, and the reflected waves blacken the photographic plate at the points where they meet it. The whole of these numerous meeting points forms the Laue-diagram.

The reflection of roentgen rays does not take place for all the wave-components which are contained in the bundle, but only for certain wave-lengths (λ), namely those for which Bragg's relation is valid.

$$\lambda = 2d \sin \alpha$$

In this equation α means the angle between the *x*-ray and the planes (glancing angle of incidence) d, the distance of two neighboring planes, the so-called lattice constant of the crystal. That is to say, a crystal chooses and reflects from a mixture of waves only those wave-lengths which bear the above-mentioned relation to the lattice constant and to the glancing-angle. The Laue-diagram, however, showed by the arrangement of the black points that wave-lengths of different sizes are reflected in the crystal and one could suppose that a continuous white spectrum exists, like the continuous white spectrum which one gets when analyzing sunlight. The arrangement of Laue's experiment, however, proved unsuitable for measuring the limits of this spectrum and the distribution of the intensities corresponding to the various wave-lengths in the spectrum.

Only Bragg's spectrometer gives the classical method of analyzing the roentgen spectrum. It does not use a fixed crystal and does not employ a lot of sets of planes (Laue), but the reflection is produced on one and the same set of crystal planes. By continually turning the crystal with respect to the primary ray in each case, corresponding to a certain angular position of the crystal, another wave-length is removed from the mixture of rays by reflection. In this way all the wave-lengths contained in the mixture can be reflected one after another, and the limits of the spectrum, as well as the distribution of the intensities, can be ascertained.

Experiments by Moseley and Darwin, according to Bragg's method, established besides the continuous white spectrum, still another spectrum, the so-called characteristic or line-spectrum which, as contrasted with the continuous spectrum, is not polychromatic, but monochromatic; that is to say, it consists only of waves of a single wave-length. The experiments proved that these waves of the characteristic spectrum are dependent on the nature of the anticathode metal. They are the shorter in wave-length, the higher the position of the anticathode metal is in the periodic system of the elements. So this second spectrum is, on the one hand, dependent on the nature of the anticathode; on the other hand, experiments have proved that the conditions of its origin are also connected by rules with the quality of the continuous spectrum. This last fact must be especially mentioned, as conclusions can be drawn as to the nature of the line-spectrum, from the analysis of the continuous spectrum, if the anticathode is known. In other words, the qualitative analysis of a roentgen radiation can rest on the analysis of the continuous spectrum alone, under the condition that the metal of the anticathode is known.

THE NATURE OF THE CONTINUOUS SPECTRUM

De Broglie was the first to analyze the continuous spectrum by Bragg's method, registering the reflected intensities of the single wave-lengths on the photographic plate; the black bands he obtained brought proof of the strictly continuous course of the spectrum for the first time; but, as Wagner has proved in detailed analytical experiments, they do not even approximately give the real course of intensity of the roentgen radiation in the spectrum, because, on the one hand, the long wave-length parts produce, on account of their greater absorption, blacknesses which are more intense than corresponds with the relative intensity with which these waves are contained in the spectrum. On the other hand, some bands which are sharply marked toward the long wave side of the spectrum and come from the selective

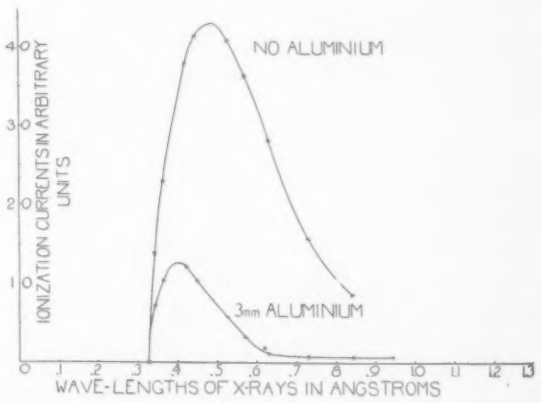

FIG. 1.

absorption of the Ag and the Br, are still to be found in the blackness of the photographic plate. By these bands the real course of the distribution of intensity is still more distorted. To analyze the spectrum Duane and Hunt made use of the ionizing method which very nearly approaches to the real distribution of energy. These exceedingly important experiments laid the foundation for rules which are equally important in theory and in practice. For these reasons we will give them in order.

1. The first experiments of Duane and Hunt led to the fundamental discovery that the continuous spectrum is sharply marked toward the short wave-length side with a shortest wave-length λ (radiation head, minimum wave-length) (Fig. 1).

2. Duane and Hunt showed that from this beginning of the spectrum the intensities of the following longer waves rise

precipitously to a rather sharply marked maximum, that the intensities then diminish more slowly toward the long wave-length side and fall off asymptotically toward the end, beyond the limit observation.

3. They found that a certain potential, V, corresponds to the shortest wave-length λ; by changing the potential, V, they found a displacement of λ in the spectrum, according to a simple rule which is called "Duane and Hunt's law of displacement." The higher the potential one applies to the tube, the more the short wave-length boundary is displaced toward the side of the short waves. This was in keeping with the roentgenological knowledge that the more short wave-length components appear in the radiation, the higher the tension applied. Duane-Hunt's law now says that the product of the shortest wave-length and the potential is a constant K, whose value is 12.300. That is to say, if the potential is known, then the shortest wave-length of the roentgen radiation is also defined; and vice-versa: if the shortest wave-length is known, the potential is given which has produced the roentgen radiation.

The simple formula for this rule is $V.\lambda_o = V\frac{c}{\nu} = K$ and is in exact agreement in the form $V = \nu_o\left(\frac{K}{c}\right)$ with the Planck and Einstein's quantum equation: $eV = h\nu$. The result of this is that $\frac{K}{c} = \frac{h^1}{e}$.

By these researches the quantum character of the radioproduction in the anticathode was cleared up at once. We mention this for the reason that we shall see farther on that March could deduct important conclusions from it as to the distribution of energy and the formation of the radiation.

Duane and Hunt's law of displacement was corroborated several times by experiments; A. W. Hull and Rice followed its accuracy up to 100,000 volts; Dessauer and Back, up to a tension of 170,000 volts. The last two authors found, for instance, at 170 kilovolts the minimal wave-length = 0,07055 AE, while Duane-Hunt's law requires the value λ_o minimum = 0,072 AE. The agreement of the corresponding values of λ_o proved even exact enough to carry out accurate measurements of Planck's radiation constant h.

4. Müller has demonstrated that Duane-Hunt's law of displacement is valid, independent of the metal of the anticathode; Wagner has confirmed this fact.

5. Ulrey has found out that the total intensity issuing from an anticathode increases with the second power of the tension (V^2), according to previous knowledge.

6. The same discoverer has ascertained that the form of the spectral course of intensity is extraordinarily similar, in practical sense the same for all metals of the anticathode.

7. Ulrey has further shown that the absolute intensities from the anticathode under the same conditions rise proportionally to the atomic number of the anticathode metal.

The most important rules of the continuous spectrum are thus shown.

8. March has given a well-considered theory for the formation of the spectrum of the roentgen radiation in the anticathode, and fixed its laws. Duane-Hunt's law has led March to suppose that the fate of the electrons, when putting on the brake, and also the formation of the continuous spectrum, is regulated by a process determined by laws. First he found out that the spectrum's form, that is to say, the distribution of energy, is chiefly determined by the fate of the fastest electrons, as the quantity of the transformed roentgen energy decreases as the fourth power with the diminution of the speed of the electrons which strike the anticathode. The fastest moving electrons are those which correspond to the vertex value of a current tension curve. If such an electron collides with the electrons and the positive nuclei of which the anticathode metal is certainly built up, its energy E, according to Planck and Einstein, is transformed into a radiation of such vibration number ν, that $E = h\nu$; that is to say, the produced wave is the shorter,

[1] V = potential; ν = maximal number of vibrations per second; K = constant.
Velocity c = velocity of light (3.10^{10} cm./sec.).
e = charge of the electron ($4.774.10^{-10}$ e.s.)
h = Planck's constant (6.52×10^{-27} erg. sec.).

the vibration number the larger, the greater the energy which the electron loses in a single collision.

(*a*) Should the electron be stopped straight off, at a single impact, its loss of energy is the greatest one can have, and the wave corresponding to the tension is formed with the largest vibration number, which is precisely the shortest wave-length, λ_o with which the spectrum is sharply marked off.

(*b*) But if the electron must go through several or many collisions until it stops, then at every collision it gets rid of part of its energy E, that is the energies E_1, E_2, E_3, etc.; these part energies, too, transform themselves into radiation and into radiation of such vibration numbers that Einstein's relation is fulfilled. We have, therefore, $E_1 = h.\nu_1$, $E_2 = h.\nu_2$, etc.

These waves ν_1, ν_2, ν_3, must all have smaller vibration numbers (larger wave-lengths) than the boundary wave-length, λ_o. They range, therefore, in the part of the curve beyond the minimum. From this can be seen that a single electron can produce either only one, namely the wave-length λ_o, or several longer waves than λ_o. The manner of the arrangement of the longer waves, in the spectral region, March recognized as taking place according to the laws of probability. That an electron loses its whole energy in one collision and the shortest wave-length is formed, will only seldom be the case, in comparison to those cases in which, in the regular structure of the anticathode, the longer waves form themselves in a *series* of collisions, that the energy transformed into radiation has a certain value E. The oftener this is the case, the greater will the intensity be with which the wave-length coordinated by Einstein's equation is contained in the spectrum. The curves calculated by March agree exactly with those found by experiment.

From these statements follows that not only the minimum wave-length, but also the distribution of energy in the spectrum can be deduced from the tension applied to the tube. In other words: by the statement of the wave-length λ_o *every radiation gets its definite qualification*. This is very important in practice, as from it follows that the chief object in spectrometric analysis is to ascertain the size of the wave-length λ_o. This wave-length is that property of the radiation which gives it its character; if the metal of the anticathode is the same, then radiations of equal minimum wave-length are qualitatively equal to one another. If the metal of the anticathode is different, then the radiations differ through the linear intensive wave components of the characteristic spectrum, the quality of the continuous spectrum is the same. Therefore, the object of this practice in any quantitative use of a radiation is to pay attention to the nature of the A.C. metal and to ascertain separately the working effect (radiographic, biological working effect, dose quotient) of the radiations of platinum and tungsten or any other metal used as A.C.; the special quality, however, can always be determined by the wave λ_o., because the characteristic spectrum bears a definite relation to it.

THE ROENTGEN SPECTROMETER BY MARCH, STAUNIG AND FRITZ

1. *Physical Arrangement.* While Laue's method makes use of the inner planes of a fixed crystal, Bragg's method uses a superficial plane of cleavage of a crystal which is continually turned, and our scheme makes use of the third possibility of arrangement, that is, the reflection of a bundle of roentgen rays on the inner planes of a rotating crystal.

From this arrangement several advantages are gained immediately. The inner planes are untouched by outside influences, as contrasted with the outside planes, which are usually defective and damaged; their ability to reflect is more perfect. The use of a relatively thinner crystal has thereby no greater influence on the absorption of the penetrating ray bundle than the absorption which takes place if reflected according to Bragg's method. By our transferring the turning point of the crystal into the optical axis, that is, into the plane of the bundle of roentgen rays, every difficulty of centering the crystal is abolished, because in every position of the crystal that plane always sets in automatically which sorts out the

corresponding wave-lengths from the mixture of roentgen rays according to Bragg's relation, as far as that wave is contained at all in the mixture.

As we are not bringing the reflected waves of the continuous spectrum to absorption on the photographic plate, but observe them in the fluorescent effect on the screen, we gain:

1. Direct insight into the roentgen spectrum, and can observe it without loss of time, without the roundabout way of developing the photographic film in the dark room and the sources of mistakes connected with it.

FIG. 2.

2. We can look into all parts of the spectrum for any desired length of time and can repeat the examination as often as we like.

3. The examination of different radiations of an apparatus, as is often necessary in practice, can be done immediately, and the whole productive power of an apparatus can be revealed within a quarter of an hour.

4. Through a particularly simple measuring mechanism we can ascertain the size of the shortest wave-length with great accuracy and read off directly from a scale in Angström units. The great exactness for reading is due to the fact that this shortest wave-length is ascertained, not from its distance from the primary bundle (which always has a considerable finite extension), but from the distance of the shortest wave-lengths of the two symmetrical spectra.

5. The great sensibility of the adapted eye allows the observation of the spectrum in the dark room (or with help of a cryptoscope in daylight) with amply sufficient accuracy.

6. The reproduction of the spectrum on the screen prevents the mistakes which are connected with the photographic plate, and allows a direct insight into the distribution of intensity of the spectrum, because the brightness of the interval sorted out in each case appears in proportion to the real intensity.

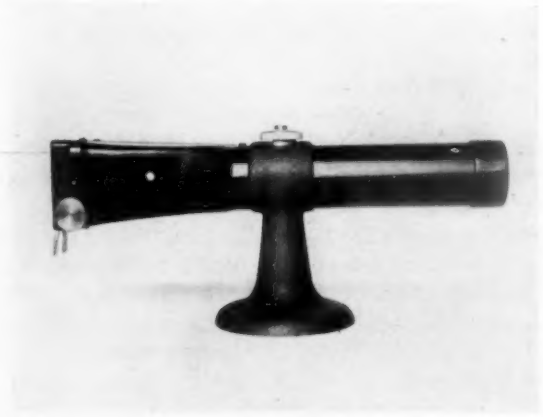

FIG. 3.

Of all these advantages, the possibility of exact observation of the sudden appearance and disappearance, and the measurement of the absolute size of the boundary wave-length is, by far, the most important, because with the knowledge of this wave-length, as we have seen, the qualitative analysis of a radiation is exhausted.

The technical arrangement of the instrument[1] which is enclosed in a small ray-tight box, can be seen in the drawing in Figure 2.

II. *Practical Utilization of the Spectral-analytic Method.* The spectralanalytic method has given a simple characteristic for every roentgen radiation in form of the size of the shortest wave-length, λ_0, which can be found easily. With this designation

[1] The instrument may be obtained through the Spectrometer Vertriebsstelle, Zentral-Röntgeninstitut Innsbruck.

each single radiation, if the anticathode is known, is sufficiently characterized for practical purposes and defined according to its quality. Herewith a way is found which leads to fruitful results for practical roentgenology, for the definition of the quality of a radiation is of great importance for radiography, for the radioscopic method, for the use of the radiation in depth therapy, for the measurement of the effective tension and for judging the work done by the apparatus and roentgen tubes.

III. *Utilization in Radiography*. Experience has proved that each radiogram needs certain conditions of exposure in order to attain its best quality. These conditions of exposure are different for every thickness of the object and density of a part of the human body which is to be radiographed. If they are not observed the well-known poorly exposed plates result which we know as the "too hard" and "too soft" plates (mistakes of penetration or hardness) and the plates with too little or excessive blackening (under-exposure, over-exposure). Up to now the practitioner had to help himself with an empiric method to get the right values of exposure for all typical and atypical radiograms, because there was no sufficiently exact method of assuring the quality of a radiation. The practitioner had to create for himself by way of empiricism, a system of exposure with the help of a systematic procedure, for the apparatus he used; he had to make experimental exposures and, by slowly eliminating the errors in exposure, to try to obtain better and better plates and finally to get a perfect system of exposure for his apparatus. Now the testing of the apparatus has become much simpler, because with the aid of such a system of exposure empirically worked out, we could find out qualimetrically (by giving the boundary wave-length, λ_o) and quantimetrically (by giving the necessary milliampere second product) the conditions of exposure for all practicable adjustments in use.

The new process of testing an apparatus is now accomplished in a much simpler way without the help of the empirical method; the radiations of the apparatus with the different settings are analyzed one after another as to their shortest wave-length λ_o, and this wave-length and the occasional milliampere deflection are registered (this procedure for a normal type of roentgen apparatus takes up a quarter of an hour). With the help of the tables worked out the densities of the different objects and the time values for exposure are added to the ascertained wave-lengths within a second quarter of an hour. That is simply done by dividing the milliampere-second product which is to be found in the table for every radiogram, by the number of milliamperes which are obtained at the proper setting. According as short-timed exposures are wanted, or longer lengths of time, one can get several systems of exposure. The roentgenograms obtained are free from exposure faults. The lengths of exposure have been fixed by us so far, for the Lilienfeld tube and the ion tube. It would be worth while, if they could also

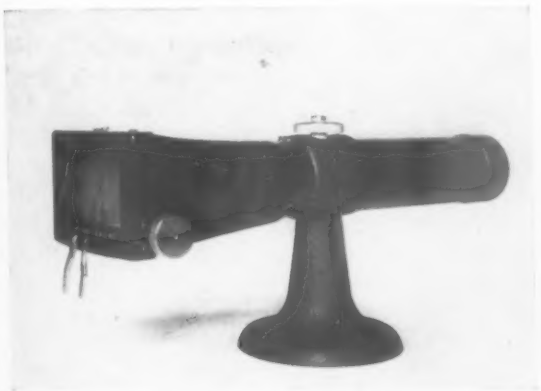

FIG. 4.

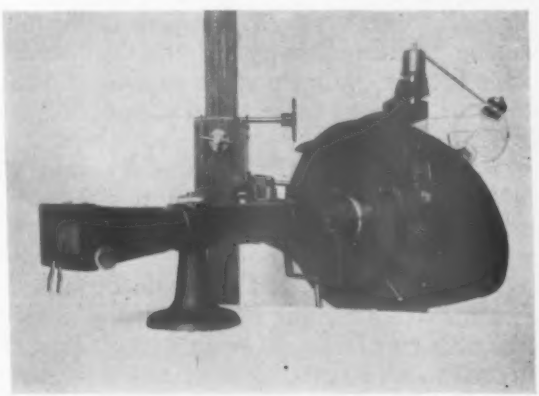

FIG. 5.

be ascertained for the Coolidge tube which is not at our disposal in Innsbruck. The tables accompany each spectrometer on delivery.

With the help of these experiments Fritz has discovered important qualities of the roentgen tube; there exist considerable differences between the amounts of current according to whether the ion tube or the Lilienfeld tube be used. To gain exactly the same photographic effect by the gas tube as by the Lilienfeld tube, at a given boundary wave-length, λ_o, the Lilienfeld tube needs double the number of milliampere seconds under the same conditions otherwise.

IV. *Utilization of Spectrometric Analysis for Roentgen Therapy.* Roentgen radiations can be used in therapy either as unfiltered or as filtered radiations; as unfiltered they are subject to the same qualitative definition as in radiography, namely, they are marked, by giving their boundary wave λ_o, as hard and soft complexes if the anticathode is known. This fundamental marking is valid in the same way for the filtered complexes, too, with the single difference that for these the kind of filtering (density and nature of the filtering metal) is still added to the statement of the *boundary* wave-length. First of all, it is clear that every radiation is connected with a certain effect on the surface and in the depth of the body. Until now it has not been possible to grasp this effect with sufficient accuracy because the quality of the radiations could not be surely defined. Only now, as enough accurate quantitative measuring methods are known, can the fixed standard of effects be allotted to certain quantities of a certain radiation. So for instance, for every unfiltered and filtered radiation, the allotted toleration dose for the skin is fixed once for all. Even the tolerance of the deep-lying normal organs of the body can be drawn into the region of observation. The biological effects which are produced on superficially situated pathological processes by different doses of the qualitatively defined radiations can only then be thoroughly studied. Above all, however, for deep therapy a most important point has to be considered: *in the case of a given quality of radiation the percentage of the dose reaching a certain depth can be fixed once for all by empiric measurement.* To measure these dose quotients for all radiations which are to be considered in therapy, and those for the different sizes of field and focus distances, was the goal for experiments which are now nearing their completion. It is easy now to find out the dose quotient for a diseased organ lying at a certain depth, because the tables worked out give the percentage which penetrates to the inner part of the body from a superficial field for every depth. The roentgen therapist, by choosing a suitable number of fields and by choosing a suitable geometrical system, is now enabled to achieve the necessary depth dose. Only now has the radiation become a therapeutic method, whose composition is known, and which can be given even quantitatively. The biological effect in the diseased organ itself and the general effect on the body only now enter into a reliable relation to the dose, and it will be the problem of the future to examine the many various results of empiricism by a more exact method of approach in this respect.

V. Measurement of Tension. For a long time it has been a well-known fact to every practitioner that the hardness of a radiation rises with increasing tension; Duane-Hunt's law has now shown that a surprisingly simple connection exists between potential and boundary wave-length. The laws revealed by Ulrey and March enlarge this connection still more, as they have proved the dependence of the quality of radiation on the potential. Therefore, the roentgen spectrometer by March, Staunig and Fritz can also serve as an absolute voltmeter; this qualification for the mentioned purpose is of great importance on this account, as the spectrometer measures just the tension which was led to the production of the radiation, therefore, that tension which alone is of value for the practitioner; for him it is insignificant how much tension the electrotechnical instruments show, if he cannot draw a reliable conclusion as to the quality of the radiation from the indicated number of volts. This he cannot do really; for on the one hand the tension is subject to great losses,

especially if the values are high, losses which occur on the way between the source of tension and the electrodes of the tube; on the other hand one must not forget that every roentgen tube is a transformer of energy which can only exploit the potential up to a certain limit. Judging by the results achieved up to now, this limit seems to be different in magnitude for the different types of tubes. Therefore, in the spectrometer a means is given which enables even a practitioner to test the real efficiency of his apparatus. For him, only the wave-length is to be considered which the apparatus can produce at the short wave end of the spectrum. From it he can draw a reliable conclusion as to the effective tension, and any other tension is of no essential interest to him.

SUMMARY

Summing up the results of the spectrometric method in practice gained so far, we can say that the new process has already led to a far-reaching uniformity and a simplification of the methods in roentgenology. Although this method has been in use for only a relatively short time, still it can already be seen that it is of importance for all branches of radiology, and there is hope that its further development will justify the expectations. Of course, there will be no lack of objections to this method in the future. We believe that, especially at the beginning, the schematical process, as we have tried to give it, will meet with opposition because the objection may be raised that the *discharge of the tension curve*, which so far was thought to be of such great importance for the quality of the produced radiation in the different types of apparatus, cannot remain without considerable influence on the relative composition of each radiation, therefore on the curve of the spectrum. It really is the case that other forms of the tension course belong to the different types of tubes, the ion-tubes on one hand, and the Lilienfeld and Coolidge tubes on the other hand, since with the ion-tubes, it has to do with the appearance of an explosive tension which is absent in the electron-tubes. Therefore, on account of the different discharges of tension, for these types of tubes, the necessity has already resulted, of separately ascertaining the quantitative working power for each of the same, with regard to the emitted radiation. But it has been shown that even for these types the *quality* of the radiation, that is, the form of the curve of the spectrum, is not obviously different. On the other hand, the results of testing, gained up to now, and the theoretical calculations by March have shown that an influence—considerable for practice—of the current tension curve on the distribution of intensity, as has been ascribed to the different types of *apparatus*, is not due to them, or only in so small a degree that it is to be neglected for practical utilization of the radiations.

STANDARDIZATION OF IONIZATION MEASUREMENTS OF INTENSITY AND MEASUREMENTS OF SCATTERED AND SECONDARY X-RAYS EFFECTIVE IN PRODUCING AN ERYTHEMA*

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THIS paper is a preliminary report of some data obtained in the study of the intensity of penetrating *x*-rays as measured at various depths in a phantom of water. The reason for giving it is to provoke a discussion which we hope will result in certain standards of measurement of *x*-ray intensity being adopted.

We were interested in knowing the penetration of *x*-rays produced by our high voltage *x*-ray machine. The literature on measurements of intensity as delivered by *x*-ray bulbs was reviewed and comparisons were made of the intensity that we obtained for 200,000 volts, maximum, filter 1.3 mm. Cu. and 1 mm. Al. at 50 cm. from the target to the surface of the water, which is similar to Dr. Dessauer's set-up, was .137; while Dr. Dessauer reports that the coefficient of absorption of water, used in arriving at the value of 55 per cent was .140. This shows that the average wave-lengths that we were using were as penetrating as those used by Dr. Dessauer. In other reports of penetration measurements, the technique of the set-ups was such that we were unable to make checks. These

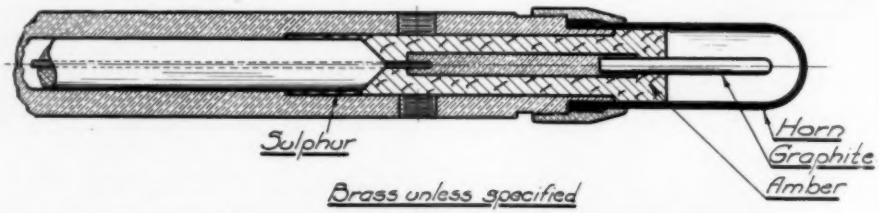

FIG. 1. Modification of Kroenig and Friedrich's ionization chamber.

reaching a depth of 5 and 10 cm. below the surface of a phantom of water. Of all the work we have been able to review, the various investigators disagree among themselves as to percentages of intensity reaching a given depth in a phantom of water, having used the same set-up. We have tried checking some data that we found, but have been unable to obtain comparable results. In some instances our penetration percentages differed widely from those reported. We made a similar set-up to one used by Dr. Dessauer in reporting 55 per cent of the surface intensity 10 cm. below the surface. Our measurements gave us 43.5 per cent when we measured the surface intensity with the ionization chamber half submerged in a phantom of water. The average coefficient of absorption of water inconsistent penetration experiments led us to investigate the variations, if any, of the surface intensity. Since penetration percentages are obtained by comparing the intensity on the surface to the intensity at given depths, it is necessary that we all have the same starting point. We will show that it is important to know definitely how the surface intensity is measured.

Our ionization chamber was modelled after one used by Friedrich and Kroenig, except for minor changes. We chose this style of ionization chamber with the view of comparing our results with those reported by Friedrich and Kroenig, and because, on account of its size, intensity changes with small changes in position can be determined. Figure 1 shows the ionization chamber drawn to size. Figure 2 shows a schematic diagram of measuring

* Read at the Midwinter Meeting of the Eastern Section of THE AMERICAN ROENTGEN RAY SOCIETY, Atlantic City, N. J., Jan. 25–27, 1923.

apparatus as set up. The ionization chamber is placed under the center of the x-ray bulb in an open field, and the electrode connected to a wire in a flexible metal tube going through an 18-in. brick wall to an electroscope with a ground glass screen for observing the image of the gold leaf of the electroscope as projected by a lens. This separation by a brick wall keeps stray radiation from the electroscope and protects the operator in making measurements. The ionization chamber, flexible cable and electroscope are sufficiently well insulated to make possible

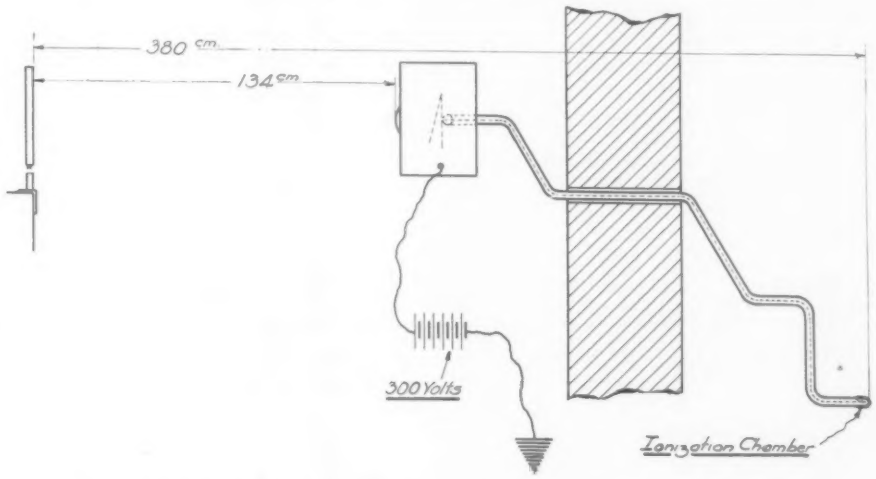

FIG. 2. Schematic arrangement of apparatus for measuring x-ray intensity.

measurements disregarding the natural leak of the system. The leak of the system is such that when charged to about 300 volts, the leaf of the electroscope will change its position on a screen 134 cm. distant at the rate of 1 cm. per hour and a half. As the time for the leaf to travel the same distance while taking readings is about 50 seconds as a maximum, the natural leak is less than 1 per cent. The electroscope was repeatedly checked with radium and found to be constant in readings to within .4 per cent as checked by various operators.

Our set-up was the same in all our work, consisting of 200,000 volts, maximum, 4 ma., 0.62 mm. Cu. and 1 mm. Al. (filter placed 30 cm. from target) and the distance from the target to the water surface was 50 cm. Figure 3 shows the various positions taken by the ionization chamber while readings are being made. The diameter of the cap of the ionization chamber is 12 mm., so that when the cap is placed on the surface of the water, the center line of the electrode is 6 mm. above the surface. When the cap of the ionization chamber is half submerged, the center line of the electrode of the ionization chamber is in line with the surface of the water. The distances 5 cm. and 10 cm. as shown in Figure 3, refer to the measurements made from the surface of the water to the center line of the electrode of the ionization chamber.

Our method of taking measurements is to start out with the ionization chamber in air, and then to place the ionization chamber on the surface of the water, half submerged, fully submerged, 5 mm., 5 cm. and 10 cm. below the surface of the water. The path of the ionization chamber is then reversed as 10 cm., 5 cm. and 5 mm. below the surface, fully submerged, half submerged and on the surface of the water, and then in air. Two readings of each position are taken and these two sets of readings averaged. This makes at least four readings of each position. Twenty different determinations of percentages have been made for each position, making in all eighty readings for each position. Assuming 100 per cent for fully submerged, the position having the greatest intensity, the following is obtained:

Position	Percentages Variation, Per Cent
In air	66.4
On surface	95.7
Half submerged	98.5
Fully submerged	100.0
5 mm. below surface	97.7

It is seen that 33.6 per cent of the intensity is due to the scattered and secondary radiation produced by the water. The chamber half submerged in water, which means that the center line of the electrode is in line with the surface of the water. However, we see that there is a 1.5 per cent greater value fully submerged than half submerged, and a 4.3 per cent greater value fully submerged than on the surface. The ionization chamber going from on the surface of the water to fully submerged has moved away from the target a distance

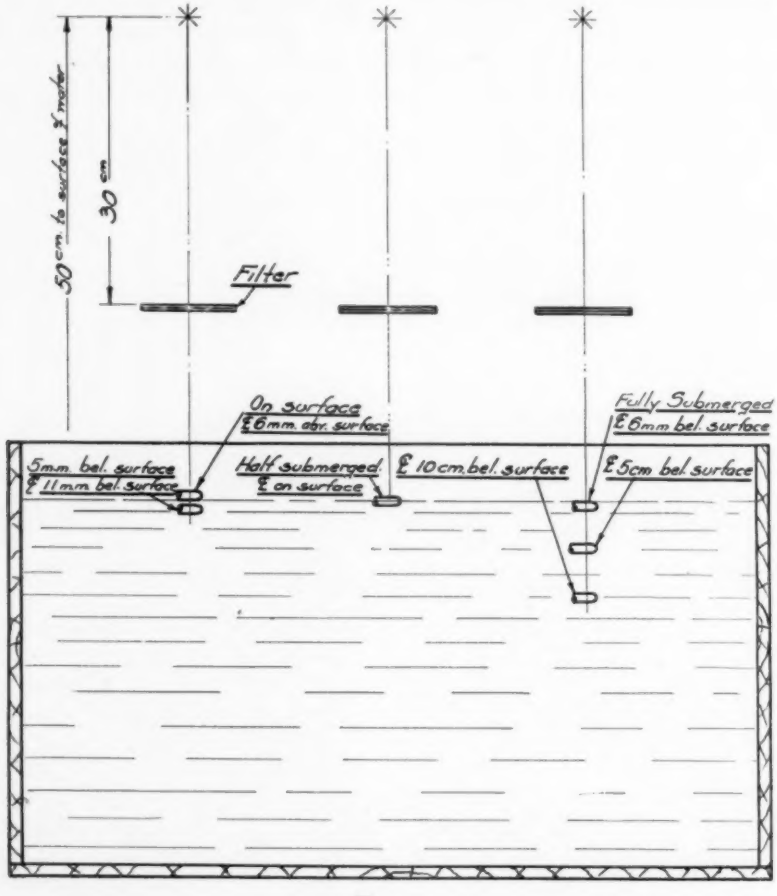

FIG. 3.

intensity of *x*-rays on the surface, then, is not only incident radiation in air but the scattered and secondary radiation produced by the water, or tissue, effective in producing an erythema. The variation on the surface may account for some of the variable results that are found in penetration experiments. What value are we to assume as our 100 per cent, or the intensity of *x*-rays effective in producing an erythema? When we began measurements, it seemed reasonable to place the ionization of 12 mm. and yet gives 4.3 per cent higher intensity. From the inverse square law, an increased distance of 12 mm. should decrease the intensity from 100 per cent on the surface to 96 per cent, fully submerged; but we actually get 104.3 per cent. There is then an increase of radiation of 8.3 per cent due to scattered and secondary radiation which is effective in producing an erythema. There is also an actual increase of intensity of 4.3 per cent from the surface of the water to fully submerged and if

penetration experiments are reported without giving the exact position of the ionization chamber, both on the surface and at various depths in the water, it is impossible for any other experimenter to check the reported penetration percentages. The dimensions of the ionization chamber will also cause a variation and should be reported. If these data are not given, the value of a report is greatly reduced, and in fact the data might be misleading.

We have made some computations of ten series of measurements showing with the same data that four different values can be obtained for the percentages of *x*-rays reaching 5 cm. and 10 cm. below the surface of water (5 and 10 cm. referring to the distance from the surface of the water to the center line of the electrode inside the cap of the ionization chamber). In making these computations, we have considered the incident radiation measured in air, on the surface, half submerged and fully submerged each as 100 per cent and then computed a corresponding percentage of intensity at 5 and 10 cm. below the surface for each of the positions considered as 100 per cent. The following table, shown by Figure 4, gives the results:

Assuming 100 Per Cent for the Following Positions	Percentages Obtained when	
	5 Cm. Below Surface, Per Cent	10 Cm. Below Surface, Per Cent
In air	108.1	60.2
On surface	74.9	41.8
Half submerged	73.7	41.0
Fully submerged	70.7	39.4

There are enough variable factors in deep *x*-ray therapy which will remain more or less variable, but here are variables which can be standardized. Recently, in conversation with Professor Russ, Physicist to the Middlesex Hospital, London, he expressed himself as being in favor of some set of standards of intensity being established. If standards are established and we all cooperate, penetration percentages are going to agree more nearly and we are all going to be able to understand each other. Such an outline of standards might be as follows:

1. The quality and dimensions of the ionization chamber.
2. The phantom to represent the tissue.
3. The exact position of the ionization chamber on the surface of the phantom in determining the 100 per cent intensity measurements.
4. The exact position of the ionization chamber in a phantom when making

Position	Variation % intensity on the surface in percent, assuming Fully Submerged 100%
In Air	66.4 %
On Surface	95.7 %
Half Submerged	98.5 %
Fully Submerged	100 %
5mm below surface	97.7 %

Assuming 100% for following position	Percentages obtained when	
	5cm below surface	10cm below surface
In Air	108.1 %	60.2 %
On Surface	74.9 %	41.8 %
Half Submerged	73.7 %	41 %
Fully Submerged	70.7 %	39.4 %

FIG. 4.

depth measurements, that is, whether the measurements refer to the surface, or center line of the ionization chamber.

About fifteen years ago, the Illuminating Engineers of America were in about the same position as the roentgenologists are to-day. Their standards of measurements were somewhat similar from one industrial firm to another, but there was just enough difference between the standards used by each to make a serious difficulty in understanding each other. About 1910, the Illuminating Engineers met and drew up a set of standards. There soon followed what is called Mazda Service, an organization for keeping each firm in touch with various improvements from one factory to another, providing

of course the firm happened to be a member of this organization. There has grown out of this close cooperation, increased business, more efficiency and better lamps.

Summing up the points that we have tried to bring out:

1. It has been shown experimentally that there is an increase in incident ray intensity as falling upon the surface of water over the incident ray measured in air, of 33.6 per cent, when measured with the ionization chamber just under the surface of the water. This increase in intensity must be taken into consideration in representing surface intensity, or intensity of *x*-rays producing an erythema.

2. It is shown experimentally that there is a variation of intensity measurements as recorded by an ionization chamber going from the surface of a phantom of water to fully submerged.

3. It has been shown experimentally that due to the variations of the surface intensity, there is a variation in penetration percentages.

4. These variations in penetration percentages can be overcome by the adoption of standards governing the quality, size and position of the ionization chamber in making measurements in a standard phantom.

DISCUSSION

Mr. Failla. Mr. Weatherwax has just shown you that variations of several per cent occur in the results of ionization measurements by the mere choice of one position or another for the ionization chamber, to represent the skin dose in a water phantom. Which position should be taken to approximate as closely as possible the radiation which is effective in producing an erythema depends more or less on the personal opinion and judgment of the experimenter. Differences arising from this source, however, are small and practically negligible in comparison to errors which may be present.

The problem of ionization measurements in radiotherapy may be divided into two parts: (1) One in which the object is to obtain data regarding the amount of radiation reaching different tissue depths under various conditions of treatment. (2) One in which the object is to make sure that the dose which has been decided upon is actually administered to the patient. I am of the opinion that, for the present at least, it is inadvisable for the radiologist to attempt to make his own dosage charts. It is best for him to take the data which has been published by different investigators and work out *average* dosage charts for the conditions which apply as nearly as possible to his machine. In regard to part 2 of the problem, for dosage purposes, I think some form of iontoquantimeter should be used in addition to the instruments now in use (milliammeter, spark gap, etc.). For this purpose, however, I think it is preferable to use a fairly large ionization chamber which can be attached to the tube holder and does not require a very sensitive measuring instrument. I have made an ionization chamber $8 \times 10 \times 2$ in., which fits into the filter holder of the *x*-ray machine, and even with a copper filter produces a current sufficiently large to be measured by an ordinary galvanometer. Dr. Duane used a similar device some years ago, but the galvanometer was of a high sensitivity type which requires very much more care than the one I am using now. This point is of considerable importance if the instrument is to be used in a doctor's office instead of in a physical laboratory.

Prof. Duane. I was very much interested in Dr. Weatherwax's estimate of depth dose (at a distance of 10 cm. below the surface) of something like 41 per cent. I would like to know how large the field was, and also if the natural leak of the instrument was determined with the *x*-ray tube running. If not running, the natural leak measurement is of very little importance. The natural leak must be taken with the tube running and something to cover the chamber so as not to get the direct rays from the tube.

There is no question about the advantage of a small ionization chamber for *certain* purposes. Friedrich's chamber is superior to my own on that account for certain experiments.

In regard to the use of carbon: Nine years ago I was using carbon plates for reducing secondary radiation. In regard to the question of direction of the rays passing through the chamber, the effects are, as you know, in the case of Friedrich's chamber pretty much the same whether the rays go through the side or end. As to just what the effect would be, if the rays came from the end where the large cable is attached to the chamber, I am not quite clear. I have, of course, made a number of experiments with my own ionization chambers with the rays going through in different directions. Whether they produce greater or less current depends upon the thickness of the plates, the size, the shape, etc.

We are all very much pleased to have men from other countries come here and talk to us and give us the results of their measurements along the lines we are following here.

DR. WEATHERWAX (closing discussion). In answer to the question Dr. Duane has asked as to the measurement of leakage while the tube was running—this was not done. I do not feel that this would change the results very much, because the field that we could possibly get from above was only 16 cm. in diameter; that was the largest possible field we could get, and it certainly would not make any difference in the changes that we got on the surface of the water. It might make a difference in the depth. This point is to be tested and reported on later.

DISCUSSION ON PAPER BY PROF. WILLIAM DUANE ON MEASUREMENT OF DOSAGE BY MEANS OF IONIZATION CHAMBERS*

DR. GLASSER. I thank you very much for your kind invitation to speak before the Society here, and especially for the honor of opening the discussion on the paper of one of the foremost physicists in the *x*-ray field, Prof. Duane. It is only the fact that I was active for nearly six years in the field of both practical and absolute dosage of the shorter wave-length rays while assistant to Professors Friedrich and Dessauer, which made it possible for me to accept your invitation. My six months' stay in this country and my duties in the Howard A. Kelly Hospital in Baltimore, as well as the kindness of all the physicians in the hospitals and factories which I visited, have enabled me to become fairly well acquainted with the state of affairs in this field in America. I would like to take this opportunity to thank all those men for their kindness.

From the presentation of Prof. Duane's excellent paper you have already become acquainted with the fundamental principles of his method of measuring the quality and quantity of *x*-rays. I should like to answer briefly some questions with regard to the difference between the Duane measuring arrangement and the Friedrich iontoquantimeter. I hope that the two drawings in Figure 1 will make clear my meaning.

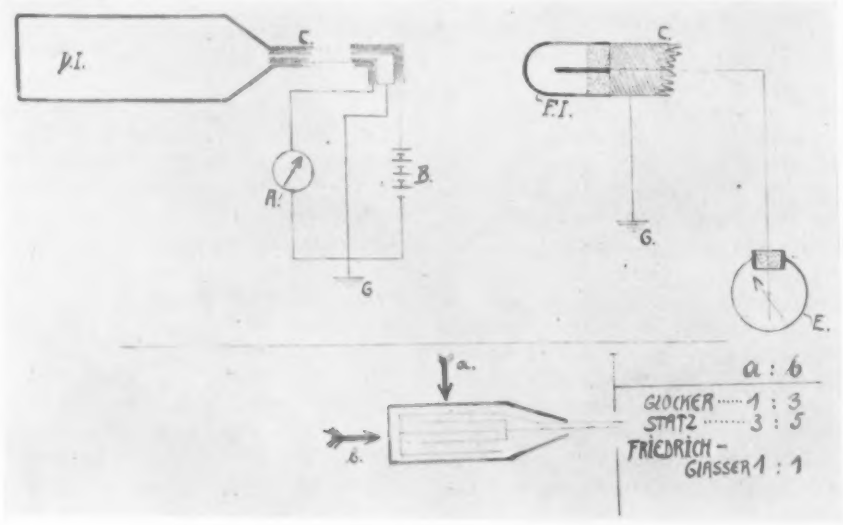

FIG. 1.

Duane measures the current passing between aluminum electrodes in the ionization chamber DI. This current is sent through by the battery B and its strength depends on the ionization in the chamber. The measuring instrument A

* This paper appeared in the May number of the JOURNAL.

is similar to a milliammeter, except that its sensitivity is about 100,000 times as great. The galvanometer circuit is completed when radiation enters the ionization chamber and then a steady deflection of the calibrated galvanometer is produced. The magnitude of the deflection, together with the time of exposure to the rays gives a measure of the dose. The advantages of the method are: (1) The arrangement is very sensitive, (2) it gives a steady deflection of the galvanometer pointer and therefore is easy of observation. Both surface and depth dose can be measured under fixed conditions, the latter only in a water phantom. The disadvantages are: (1) The use of a relatively large ionization chamber (I shall come back later to the difficulty of measuring accurately with it), (2) it is very difficult for the practical man to make proper use of the extremely sensitive galvanometer (this is borne out by the experiences of which Dr. Pfahler spoke at the Detroit meeting), (3) fluctuations in the line voltage and current cause variations in the intensity of the rays and therefore variations in the galvanometer reading; therefore the mean value of the deflection has to be taken for the calculation of the dose.

At the upper right-hand corner of Figure 1 you see the schematic representation of the Friedrich iontoquantimeter arrangement. FI is the little horn ionization chamber. It has approximately $\frac{1}{10}$ of the volume of the Duane ionization chamber. The inner electrode is connected with an electrometer which works just like an ordinary gold-leaf electroscope. This, together with the inner electrode, must be charged by means of a special charging device. On account of the ionization in the chamber the charge passes to the wall of the chamber, which is grounded; the potential of the electrometer goes back to zero. The time required for the pointer to go back gives a measure of the applied dose. The advantages of this method are: (1) The use of a very small ionization chamber which can even be introduced into body cavities. Thus the depth dose can be measured, not only in a water phantom, but in the body itself, during the treatment. (2) The less sensitive indicating instrument is much better for practical purposes; it automatically records variations in the intensity. A disadvantage is the longer time consumed to charge and read the instrument.

With regard to Prof. Duane's method I should like especially to mention the elegant manner of measuring the quality of the radiation by determining the "effective wavelength." This method is similar to the Dessauer method of measuring the absorption coefficient μ for water with the Bachem electroscope. However, Duane's method has the advantage that with the same ionization chamber he can also measure surface and depth doses. Of course these can also be measured with the Friedrich ionization chamber.

Coming to this method of measuring the intensity or quantity of the rays: In the different constructions of ionization chambers which I have made so far, I have always noticed that two things must be taken into consideration for measuring depth doses. The first of these is the question of the so-called "direction effect," which was first noticed by Glocker. Permit me to explain this statement by means of the diagram in the lower half of Figure 1. It shows a Duane ionization chamber. The rays falling perpendicularly on the chamber are denoted by *a*, those falling axially by *b*. It has now developed in most cases that radiation of a given strength falling on the chamber in the direction *b* produces a much greater current in the chamber than rays of the same strength incident in the direction *a*. (Exceptions are sphere ionization chambers.) A few comparative values are given: Glocker found for an ordinary chamber a ratio of the two measured intensities of 1:3, Statz, a ratio of 3:5 and Friedrich and Glasser, for their small horn ionization chamber, 1:1 (of course only in measuring such rays as do not run in the same direction as the feeding cable). In other words, there was no "direction effect." You will realize that it is a matter of great importance that the measuring chamber should be so constructed that radiation incident in any direction, e.g., in the case of the scattered radiation, shall produce equal ionization for equal intensity. In the case of the surface dose the main effect is produced by the primary rays, which are undirectional. In the case of the depth dose the main effect is produced by the scattered rays, which come from all directions. If the ionization chamber shows the "direction effect" the depth dose in percentage of the surface dose will undoubtedly be measured inaccurately. Standardizing of such small ionization chambers by comparison with very large chambers, which can be built without "direction effect," takes this effect of the small chamber into account only if the position of the small chamber in the body, phantom, etc., is always exactly indicated.

The second demand which is made of a chamber measuring ionization is that it have the smallest possible volume. This point was first emphasized by Dessauer, who showed that the introduction of a relatively large extraneous object into the body causes radical changes in the conditions of scattering and thus makes impossible a correct determination of the true depth dose. Figure 2 shows the results

of measurements which I made some years ago in order to determine what influence air included in the body (e.g., in the case of the lungs) has upon the depth dose. An air volume V, of about twice the size of the Duane chamber, was placed at different points in the water phantom W, and under the same conditions of radiation in each case the depth dose under the phantom was measured with a Friedrich ionization chamber, as shown. Below are shown the relative values of these depth doses. You can readily see what a great influence the position of the air volume has upon the depth dose solely through a change in the scattering conditions, for the radiation passes always through the same layer of water. It would be interesting to know whether such differences of scattering

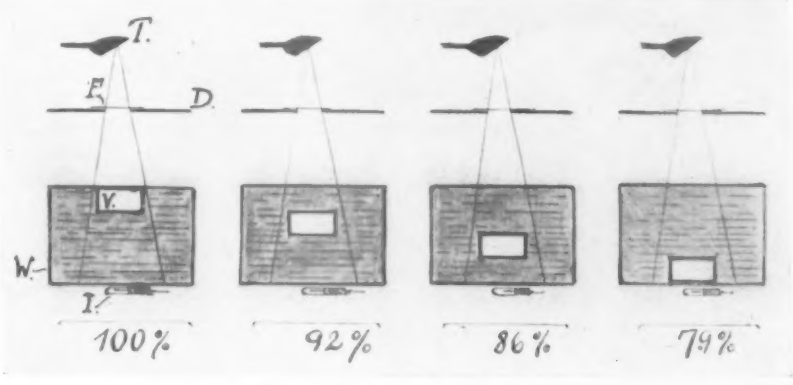

FIG. 2.

distribution occur in using Prof. Duane's chamber. Certainly, on account of the presence of the various layers of aluminum and on account of the smaller air volume, the effect will be much smaller; nevertheless it seems to me that the small depth dose of 33 per cent under normal conditions, which Pfahler measured with the Duane chamber, as he mentioned in Detroit, can be traced to the fact that this chamber did not measure the real amount of scattered radiation.

The third point I want to mention is that all the measurements with the Duane arrangement involve a fixed technique, with regard to effective wave-length, focus skin distance, size of field, etc. Friedrich wants to avoid this, and he has succeeded in doing so with the horn chamber. I am sorry that I can not go more into detail on these points. The horn chamber measures the intensity practically independent of the hardness of rays now in use. Experiments which we have made at the Howard A. Kelly hospital have shown that this independence of dose and wave-length is valid even down to

the γ-rays of radium. It must be said, however, that as rays of different hardness can produce different biological effects, it remains necessary to measure the quality of the radiation. But, necessary as this specification of hardness may be, it involves great difficulties, partly of a technical nature and partly due to phenomena which science as yet is not able fully to explain. I will only mention that, according to recent investigations by Compton and Crowther, it must be suspected that the scattered rays may have a different quality from that of the primary radiation. You can see what a great influence this would have on the measurement of the dose. Certainly in this case, as in the case of many practical questions, that ionization chamber which measures the dose correctly independent of all these factors, would be given the preference.

All in all, it seems to me that a very small horn ionization chamber has many advantages over a larger one of any other material. It serves the purpose of the physician better, in that he can introduce it even into body cavities and watch the indicating instrument during the whole treatment; the physicist will give the horn chamber the preference for the reasons enumerated above. Moreover, any small chamber, as well as a larger one, can be standardized by means of a very large one, as Duane, Holthusen, Friedrich and Glasser have shown. I have a paper in the January number of your Journal in which such standardizing experiments with a very large ionization chamber are described in great detail.

A few words as to the measuring instrument: With the small horn chamber an electroscope must be used, for even the most sensitive galvanometer would here detect practically no current. Nevertheless modern science has made it possible to measure the current of a

small horn ionization chamber, and that not with a delicate highly sensitive galvanometer, but with an ordinary instrument only a little more sensitive than a milliammeter. You all know from your radio sets the amplifying tubes which are used to magnify the small currents received by the antennæ. The same thing is possible in the case of the small current in the ionization chamber; and arrangements embodying this principle have already been employed in Europe. I would like to call your attention to such an arrangement in Figure 3, which is manufactured by the firm of Siemens and Halske. The ionization current of the small Friedrich chamber K is magnified approximately 100,000 times by the

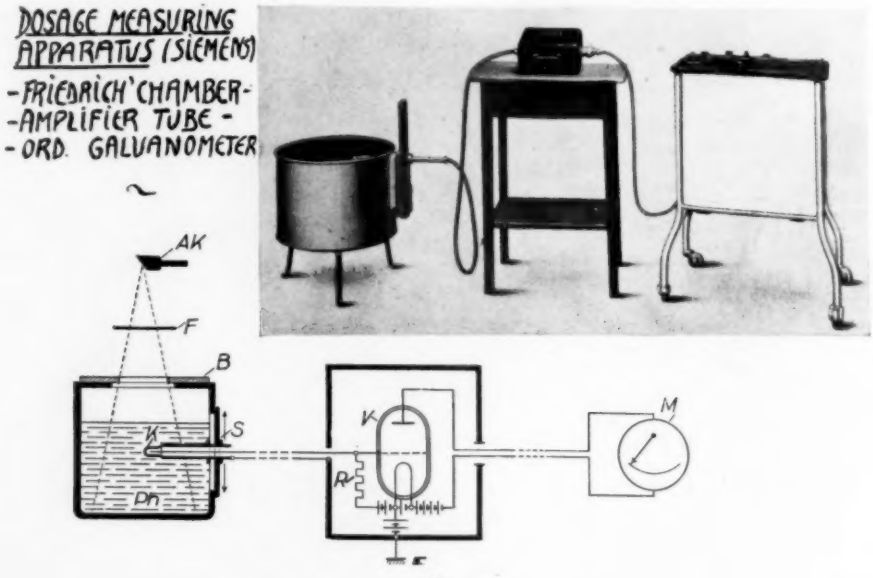

FIG. 3.

amplifier tube V, and can not only be measured by an ordinary measuring instrument, but even recorded in a diagram for the whole treatment. You will easily see the ideal combination of the small chamber and good galvanometer for practical purposes in connection with the factors I mentioned above. Since almost everyone has a radio set nowadays the use of this instrument should not be too difficult. I should like to say a few words on the biological standardization of dosage-measuring instruments in terms of electrical units. Duane's galvanometer scale is calibrated for a certain ionization chamber in terms of a certain unit, which he calls E. If he multiplies this number E by the time of exposure, he gets the applied dose in another unit *e*, that is, in the unit in which Friedrich first calibrated his iontoquantimeter seven years ago. Again I regret that I cannot go more into detail on this point. According to Prof. Duane's observations, his number *e* does not agree with Friedrich's for a given dose. His value is always much larger than Friedrich's. This fact was mentioned by Friedrich and Glasser in a short communication at the Berlin Roentgen Ray meeting in 1920. It may be mentioned here that the difference is given through the definition by both investigators. In determining dosages Friedrich measures with his ionization chamber the energy absorbed in the tissues. Duane, on the other hand, measures the energy of the original *x*-ray bundle by the absorbtion in air. The fact was also discussed at length in the paper in the January number of your Journal, of which I spoke before. Friedrich will, in the near future, publish the quantitative results. This, however, has no bearing on the practical application of the iontoquantimeter in its present form, since a definite dose corresponds to a definite number of divisions on the scale.

In conclusion, permit me to say a few words as to the attitude of the physicist toward these dosage problems. We have divided up the whole problem of dosage into two parts: practical and absolute dosage. The substance of Prof. Duane's paper and of my own words up to this point belongs mostly to the field of practical dosage, that is, methods are given for measuring the quality and quantity of the rays at every point of the radiated body. But do not get the idea which I have frequently heard expressed on both sides of the Atlantic that the physicist is the man of large doses

which he wants to apply all at once, the man of large sizes of field, focus skin distance, etc. In other words, a man who recommends applying the physical dose without taking into account the biological, anatomical and physiological considerations which play an important part in the determination of the correct dose. It is for this latter reason that the expression of absolute dose was introduced and emphasized, since it is absolutely necessary to arrive at a closer approximation to the actual biological ray effect by means of an instrument measuring this effect that is the absolute dose. It is in this field that the physicists have given us a series of valuable researches. I have recently described some of these which were carried out with the aid of the small ionization chamber. These, as well as a whole series of other biological experiments, show how much the small horn ionization chamber is to be preferred, especially in these absolute dosage measurements.

No one will deny the fact that the ultimate purpose of our dosage determinations is a biological one. But so long as biology cannot formulate its requirements exactly (and that will be the case as long as we have only such incomplete data) the best thing we can do is to measure as accurately as possible everything that can be measured; and that is the physical part of the dose. As soon as we know more on the biological side of these questions we can, if necessary, modify the physical dose.

At present the main problem for the investigator is to collect and systematize our present knowledge of the biological phenomena involved. With this as a starting point, using the tools which physics, chemistry and biology have given him, he must work toward the solution of the problem of the biological effect of roentgen radiation. If I were to look into the future from the physical standpoint, and guess at the solution of this problem, I would say that this solution will perhaps be found in the use, not of a single practical wave-length of a certain roentgen-ray bundle, but in the use of all combinations of all wave-lengths, from the longest electric waves through heat waves, light waves, roentgen waves, down to the shortest rays of radium. I am sure that in this field also the medical man must work hand in hand with the physicist, to their mutual advantage.

STEREOSCOPY OF THE ACCESSORY SINUSES*

BY G. W. GRIER, M.D.

PITTSBURGH, PENNSYLVANIA

WHILE the value of stereoscopy in sinus work is admitted by the majority of roentgenologists, I have gained the impression from visits to my friends in various parts of the country, that it is often not a routine procedure in their laboratories. Presumably, this failure to use in practice a procedure which is theoretically desirable must arise from one of two reasons: First, there is doubt as to whether its use is of real advantage; second, it is too much trouble to be bothered with as a routine method.

It is not possible for me to prove in a short paper like this the practical advantages of this method. As a matter of fact, the only way one can be convinced is to try it as a routine measure for a short period. The additional information thus obtained will be so evident that one will never be satisfied with anything else. This subject was discussed in detail by Van Zwaluwenburg in his masterly article delivered before this Society at the annual meeting in Washington in September, 1921. I can only add that I have found the method of inestimable value in a large number of cases, and that the additional information obtained and the feeling of certainty as to the conditions present are ample compensation for any extra trouble.

My object in this paper is to remind you again of the value of stereoscopy in sinus work and to demonstrate a method of making plates that always stereoscope, a method which is no more trouble to carry out than the making of flat plates. The plates so made can be viewed singly and are made in what are recognized as standard positions. The tube is shifted vertically as recommended by Van Zwaluwenburg. The angles, however, are a little different and are accurately measured.

I use the head-rest which I described before this Society at the midwinter meeting two years ago, although this is

*Read at the Midwinter Meeting of the Eastern Section of THE AMERICAN ROENTGEN RAY SOCIETY, Atlantic City, N. J., Jan. 25–27, 1923.

not essential to stereoscopy. The average time consumed in fitting the patient's head in the head-rest and making two sets of stereoscopic plates is three minutes. This is certainly not a time-consuming procedure.

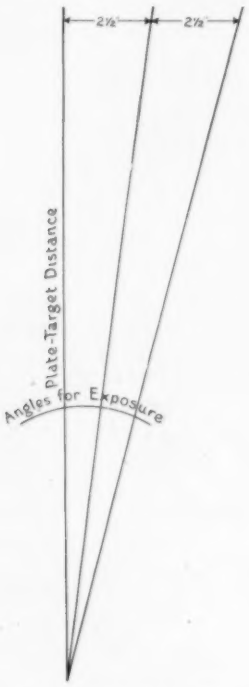

FIG. 1. Method of determining the proper angles for the different exposures.

The position which the tube shall occupy for the different exposures is determined by laying out the angles on a sheet of paper and then transferring them to a triangular sheet of aluminum with which the actual measuring on the patient is done. Since there is but comparatively little difference in the size of heads, it is practical to make sinus exposures with a fixed distance between the tube target and the plate. The procedure is easier if a small cone is used on the tube stand and the end of the cone brought a short distance from the back of the patient's head. With the tube in this position, the distance from the target to the plate is measured and this distance is used as the altitude in drawing a triangle, base up, which is to determine our angles. The base of this triangle is twice the distance between the pupils of the eyes, or 5 in. The triangle so constructed is halved by bisecting the base line. We now have the three angles at which to make our exposures, represented by the sides of the two triangles, and as the middle angle is the stereoscopic distance, or 2½ in. from either end, it will stereoscope with either.

This triangle is drawn full size on a piece of paper and the resulting angles are copied on an aluminum triangle which is fixed to the head-rest or which may be used by placing it against the side of the patient's head in the usual way. One side of the aluminum triangle is so placed as to connect the external auditory meatus and the external canthus, the point of the triangle at the canthus, the base of the triangle extending toward the forehead. The altitude of this triangle then roughly outlines the base of the skull. The first exposure, or the one made at the greatest angle, is made at an angle of 30° to the base line. This is easily done by tilting the tube so that a line drawn down lengthwise through the middle of the cone would be continuous with the line drawn on the aluminum triangle. The two succeeding exposures are made at the remaining angles in a similar manner.

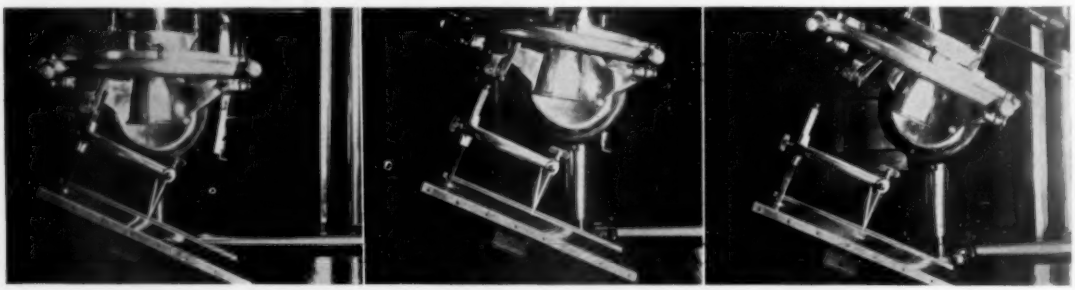

FIG. 2. Tube tilted to the different positions for the three exposures.

We thus get three plates, any one of which is a perfectly good flat plate, the middle one stereoscoping with either one of

the others, being the plate for the right eye with one and the plate for the left eye with the other. When viewed singly, the plate made at 30° shows the frontals best, and the last one shows the maxillaries best. The middle one is made at about 25° which is the universally accepted angle.

Since talking with Dr. Granger of New Orleans about his sphenoid work, I have recently added a fourth exposure made lower down, which, when stereoscoped with the third one, shows the sphenoids very nicely. I have not had time yet to draw any conclusions regarding the value of this.

In conclusion: I am sure anyone who will take the trouble to stereoscope all his carefully made stereoscopic plates. When I say "carefully made" I do not mean carefully made stereoscopically, because that is very easy as far as the angle is concerned. When I say "perfect plates" I mean technically perfect so far as detail is concerned. You need a plate in which the bony outlines of the walls of the sinuses, the ethmoid septa particularly, are clean cut, in which there has been no motion to the patient, no movement or tremble from respiratory action, and no blurring from the target of the tube

This presentation of Dr. Grier is very clever. It is well worth doing, and the effort does not require any more time than making ordinary plates. Moreover, if one has the angle permanently fixed on the tube stand, it does save

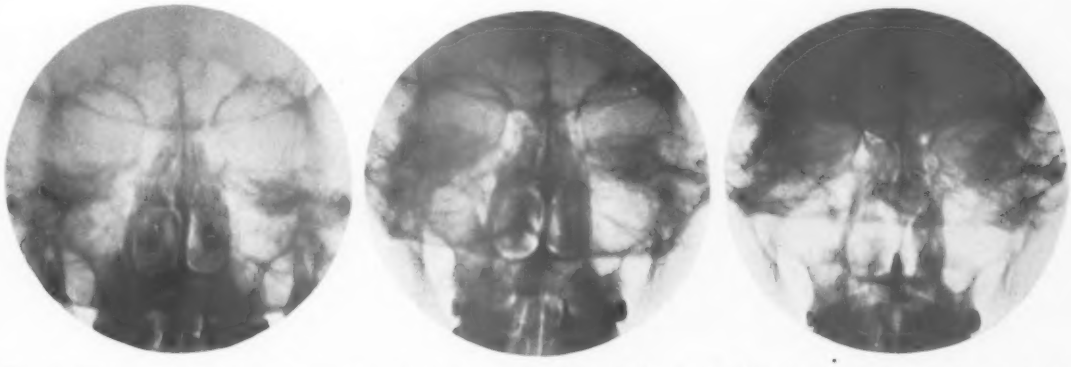

FIG. 3. The resulting views from the three exposures, the middle one of which stereoscopes with either of the other two.

sinus cases in this way, for a short time, and study the plates carefully, will quickly convince himself that the method yields information not to be had in any other way.

DISCUSSION

DR. LAW. I am glad Dr. Grier spoke of stereoscopy of the sinuses. A few years ago I used to think that it was not necessary, but in those days we were looking for gross lesions and for opacity in the sinuses.

Since the war I have given up that idea. Any case which will show opaque sinuses is fairly readily diagnosed by the surgeon before the patient comes to you, and any one can tell the difference between an opaque sinus and one which is clear. That is not the type of case that the surgeon wants the most information from. What he wants is the case that comes in with vague symptoms—headaches, postnasal discharge and some focal infection—cases in which he cannot see pus on physical examination. That is the type of case that requires trouble. I congratulate Dr. Grier on his cleverness; but he did not say anything about the real value of what he has presented.

Chronic sinusitis is really what we are trying to diagnose, and you get that only by the change in the bony structures. You put a plate in the box, look at it, and to all intents and purposes that plate is perfectly normal. The frontal sinus shows well, the ethmoid region is nice and black, apparently clean-cut and clear—a perfectly normal sinus. The same way with the antrum; but there are the indirect evidences. If you get the symptoms you find that the patient has probably a postnasal discharge, periodic headache coming on at a certain time during the day and disappearing the same way; perhaps you get some little eye trouble. Interpretation of the plates a year or so ago would have been perfectly clear sinuses, but now the signs we look for are the bony changes. Learn to study the ethmoid region. That is just as important as the mastoid region; it is the same structure and goes through the same changes. If the mastoid had as good

drainage as the ethmoid, you would not get operative mastoids as you do now. Notice the walls—whether they are absorbed or whether they are thinned out, thickened or fuzzy, and learn the appearance of those changes in chronic conditions; and you will deliver to the surgeon a diagnosis on which he can base his method of treatment and thereby help your patient.

Dr. Grier (closing discussion). It does not matter in what position you put the head in making these examinations, because you are measuring from the base line of the skull, and if you measure the angles at which you work the position of the head is of no importance.

I have purposely omitted saying anything about what we learn in these stereoscopic examinations of the sinuses for two reasons. The first is that it opens up a subject which I feel is too large to be discussed in this short paper. The second reason is that I have seen so many things since making these examinations which I do not understand. Shadows appear which I never saw before and which I am unable to interpret. It occurred to me, however, that while I was working on the question of interpretation I would report this technique, in order, if possible, to stimulate interest in this method of examination.

On account of a defect in my vision I have a great deal of difficulty in seeing stereoscopically, and unless the plates are perfectly made they will not stereoscope for me. I do not make lateral plates, because the only thing I learn from the lateral projection is the depth of the sinuses and this is not improved by the stereoscopic vision, whereas the frontal projections demonstrate the pathology present.

TRANSLATIONS AND ABSTRACTS

Hütten, V. D. Duration of cure of operated breast carcinoma with and without prophylactic *x*-ray treatment. *Münchcn. med. Wchnschr.*, 1922,i, 13.

The classification was made according to Steinthal-Anschütz, based upon the severity of the lesion in Groups I, II and III. Each group was subdivided into: A, those who had not been treated; B, those who had been treated moderately; and C, those who had received intensive irradiation. The following results are reported:

Group I

A. 23 cases.

After three years, living, free from recurrence, of 19 cases, 19 (100 per cent).

After five years, living, free from recurrence, of 12 cases, 12 (100 per cent).

After ten years, living, free from recurrence, of 5 cases, 4 (80 per cent).

B. 3 cases.

After three years, living, free from recurrence, of 3 cases, 3 (100 per cent).

After five years, living, free from recurrence, of 2 cases, 2 (100 per cent).

C. 6 cases.

Term of observation too short for adequate observation.

Group II

A. 39 cases.

After three years, living, free from recurrence, of 35 patients, 3 (8.6 per cent).

After five years, living, free from recurrence, of 23 patients, 2 (8.7 per cent).

After ten years, living, free from recurrence, of 5 patients, 0 (0 per cent).

B. 4 cases.

After three years, living, free from recurrence, of 3 patients, 0 (0 per cent).

After five years, living, free from recurrence, of 1 patient, 0 (0 per cent).

C. 2 cases.

After one-half year, 2 recurrences.

Group III

A. 70 cases.

After three years, living, free from recurrence, of 63 patients, 31 (49. 2 per cent).

After five years, living, free from recurrence, of 46 patients, 18 (39.1 per cent).

After ten years, living, free from recurrence, of 21 patients, 4 (19.1 per cent).

B. 8 cases.

After three years, living, free from recurrence, of 7 patients, 1 (14.3 per cent).

After five years, living, free from recurrence, of 1 patient, 0 (0 per cent).

C. 4 cases.

Recurrence in one year 2 patients: 1 living and well one year after operation, 1 living and well two years after operation.

The author on the basis of the material of the Giessen Klinik takes a similar point of view as that of the clinics of Tubingen and Leipsic that through the after-treatment with *x*-ray the result is made worse.

THE AMERICAN JOURNAL OF ROENTGENOLOGY AND RADIUM THERAPY

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Information of interest to all readers and lists of officers of The American Roentgen Ray Society and The American Radium Society will be found on the two pages preceding Table of Contents.

CORRESPONDENCE

To The Editor:

In his very interesting and valuable article* entitled "A Biological Coefficient for the Aluminum Filter," Dr. Francis B. Sheldon proposes a law, or formula, by which the time required for an erythema dose of *x*-rays filtered through aluminum can be computed from the known values of the tube current, the spark-gap, the target-skin distance, and the thickness of the filter. This equation, which he has found to fit fairly satisfactorily both his observations and those of others for filters ranging from one to 10 mm. in thickness, can be put in the condensed form

$$mam \times G^2/D^2 = t(15 + t/2)$$

where *mam* is the number of milliampere-minutes, G^2 is the square of the spark-gap in inches, D^2 is the square of the target-skin distance in inches, t is the thickness of the filter in millimeters, and the line / denotes division. For simplicity in writing and printing, we shall hereafter denote the entire expression on the left of the equality sign by the single letter R. It is a measure of the total amount of radiation that would reach the skin if the filter were absent. The equation then becomes $R = t(15 + t/2)$. It is evident that this equation cannot be strictly correct, because it requires R to be zero when t is zero. This means that when there is no filter, an erythema can be produced without any radiation at all, an evidently preposterous claim. In fact, we know that when there is no filter, the value of R for an erythema is something like 4 or 5. This discrepancy can be easily removed by adding another term to the right side of the equation. This quantity, which we may denote by E_0, is equal to the value of R that produces an erythema when there is no filter; the equation then becomes $R = E_0 + t(a + bt)$, where a and b are quantities corresponding to the 15 and the ½ in Dr. Sheldon's equation, but differing from these in actual values. The equation we have just written is evidently equivalent to $R - E_0 = t(a + bt)$, which shows that it is not R, but the excess of R over E_0 that is determined by the thickness of the filter. This is obviously as it should be; the thickness of the filter does not determine the amount of radiation required for an erythema, but only the *additional* radiation above what is needed when there is no filter; $t(a + bt)$ is the amount, erythemally considered, by which the filter reduces the radiation.

TABLE I

5 MA. 9-IN. GAP, 10-IN. DISTANCE

Filter, Mm.	Time	
	Original, Min.	Modified, Min.
0.5	1.88	2.81
1.0	3.7	4.5
2.0	7.9	8.1
3.0	12.2	12.1
4.0	16.8	16.5
5.0	21.6	21.3
6.0	26.7	26.4
7.0	32.1	31.8
10.0	49.4	50.5

If, for the purpose of illustration, we assume that E_0 is exactly 5, the data given by Dr. Sheldon on page 307 lead to the equation $R = 5 + t(13.5 + 0.75t)$; by

* AM. J. ROENTGENOL., Apr., 1923, x, 307.

means of this modified equation the times are given in the third column of table I have computed. Excepting the two thinnest filters these differ but little from those found by means of Dr. Sheldon's original equation (column 2).

For the thinnest filters, Dr. Sheldon's formula leads to lower values; the thinner the filter, the greater the difference between the times as computed by the two formulas. That his formula probably gives times that are too low, when the filter is thin, is indicated by his recorded observations; under experiment Number 3, filter 1 mm., it is stated that the computed time gave only a slight reddening that quickly disappeared; under Number 5, filter 1 mm., we are told that for a dose nearly double (actually 1.82) that computed for an erythema there was only a reddening that lasted for six months. From the third column of Table 1, it appears that the dose for erythema is probably 20 per cent greater than that given in Experiment 3, and that the dose given in Experiment 5 was only 50 per cent greater than that required for erythema.

It should be remembered that the values computed here by means of the modified formula are merely illustrative and are based upon an assumed erythema dose of unfiltered radiation that has been only guessed at. In all such work as that of Dr. Sheldon's the erythema dose of unfiltered radiation should be determined under the same conditions as are those with filters. As it is to be expected that the effect of the filter and also that the erythema dose without filter will depend upon the hardness of the radiation, observations should be grouped according to spark-gap, and those corresponding to the several spark-gaps should be separately studied. The various groups of values thus found for the constants E_0, a and b will yield very valuable information that may be amenable to simple explanation and formulation. It may be found that terms in higher powers of t will be required under some conditions in order to express the observations satisfactorily.

As Dr. Sheldon points out, the constants found by one observer may not satisfactorily fit the working conditions of another, on account of differences in the manner of measuring the spark-gap, errors in the measurement of the current, etc. This, however, does not greatly reduce the value of the compilation of such data, as the degree of concordance, or of disagreement, that is thus revealed will give valuable and much-needed information regarding the concordance that actually exists between the practices of different radiologists. Moreover, certain types of individual idiosyncrasies in the values of a and of b may be eliminated by dividing these quantities by the corresponding value of E_0.

I hope that Dr. Sheldon will make another study of the data he has in hand, taking into consideration the suggestions here made.

N. Ernest Dorsey.

Washington, D. C.,
May 7, 1923.

To The Editor:

First I wish to thank Dr. Dorsey for his very fair criticism of my Coefficient Formula. No doubt the formula would be better with a constant for the erythema dose included. However, the value of E_0, the erythema constant without a filter, is only 2 and not 4 or 5.

Taking the figures originally given by Witherbee and Remer for their unfiltered erythema dose, 3 ma., 3 in. spark-gap, 5 min. at 8 in. distance, we get a constant of 135/64 or approximately 2. I find that this works very well with my machine. Now in using this E_0, that is 2, the formula will have to be changed to make the time for the erythema, as found in the experiments, equal that given by using the original formula. To do this we would have to use a formula something like the following:

$$mam \times G^2/D^2 = 2 + t(12 + t)$$

t being the number of mm. of aluminum used for filter, and using $12 + t$ instead of $15 + t/2$ used in the original formula.

By using this formula the time obtained up to, and including, 7 mm. will be within a very few seconds of that found with the original formula, and above that the time will be slightly increased.

Francis B. Sheldon.

Fresno, Calif.,
May 25, 1923.

DR. GEORGE HONORED

At a meeting of the Royal Society of Medicine in London, on May 17, the Sir James MacKenzie Davidson memorial medal was awarded to Dr. Arial W. George of Boston.

This is the first time that this distinction has been awarded to an American. Dr. George has for many years been an exponent of roentgen-ray diagnosis of diseases of the gall-bladder, a theory which was for a long time rejected by many co-workers. The medal which was conferred upon him is a just recognition of this work.

The presiding officer was Sir Humphrey Rolleston, president of the Royal College of Physicians, and the meeting was attended by leading members of the British medical profession.

Dr. George's subject at this meeting was "The Pathological Gall-Bladder" and the occasion of the address was the annual memorial lecture, delivered this year in memory of Sir James MacKenzie Davidson, who was among the first *x*-ray martyrs. Dr. George was also a guest at the 150th annual dinner of the Medical Society of London.

PROFESSOR DUANE JOINS EDITORIAL STAFF

The Journal is very much pleased to announce that Prof. Duane has kindly consented to serve on its editorial staff. His work in the forefront of experimental radiology is well known and is recognized as having very materially assisted in placing American radiology on a stable and scientific basis. It is therefore a matter of congratulation to all of us that he has consented to give us more of his time and advice by serving in the capacity mentioned.

THE RADIOLOGICAL SOCIETY OF MEDICINE OF FRANCE

This Society announces the following board of officers for 1923:

President: Dr. Ledoux-Lebard, 22 Rue Clément-Marot, Paris.

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Editor: M. Masson, 120 Boulevard Saint-Germain, Paris.

THE LEONARD PRIZE

The American Roentgen Ray Society is again offering the Leonard Prize in 1923, details for which appear on advertising page viii of this number of the JOURNAL. The manuscripts submitted for the 1921 prize were of a high order of merit and covered a variety of subjects pertinent to roentgenology. It is to be hoped that the contestants for the next prize will be equally zealous in their efforts.

Subscribers to THE AMERICAN JOURNAL OF ROENTGENOLOGY *visiting New York City, are invited to make the office of* THE JOURNAL (*69 East 59th Street, New York*) *their headquarters. Mail, packages or baggage may be addressed in our care. Hotel reservations will gladly be made for those advising us in advance; in this case, kindly notify us in detail as to requirements and prices. List of operations in New York hospitals on file in our office daily.*

TRANSLATIONS & ABSTRACTS

SCOTT, S. GILBERT. A method for the opaque meal examination of the stomach. *Arch. Radiol. & Electroth.*, Mar., 1923, xxvii, 304.

The author reviews the current methods of examination and gives full details of the method which he employs. He incorrectly and doubtless inadvertently states that in America radioscopy was little used until recently. He decries the multiple plate method as impracticable and expensive for a busy hospital in England and states that its diagnostic value is not sufficient to justify the expense entailed. The combined screen and plate method is the generally approved one.

The author's method includes systematic screen palpation which must not be confused with the stomach prodding or palpation used by many workers. This palpation is carried out inch by inch, starting at the cardiac and ending at the duodenal end. Such a satisfactory screen examination requires attention to many small details which the author describes. Generally speaking he feels that it is unnecessary for the radiologist to know the clinical history of a gastric case. The author has found in many difficult cases, such as a small ulcer on the anterior or posterior wall of the stomach, or high up toward the cardiac end, extreme difficulty in demonstrating the condition on a plate or film, although the diagnosis was already definite from the screen study; and he goes so far as to state that any lesion demonstrated on a roentgenogram would easily be diagnosed by screen palpation, but that the reverse does not hold good. The two chief advantages of palpation under the screen are that after a time it can be carried out with rapidity; and that it is extremely necessary in the detection of small lesions. Errors in diagnosis, if traced to their source, are of extreme value. Every error in diagnosis should be thoroughly investigated. It is a mistake to slur over one's mistakes.

The author administers a vegetable purgative thirty-six to forty hours before the examination. He prefers castor oil. The last meal, which must consist of fluids, should not be taken less than an hour before the examination. The object of this preparation is to have the intestinal tract as empty as possible, while at the same time avoiding artificial conditions; that is to say, the effect of the aperient must have passed off. The patient should not feel unusually hungry, as this condition has a definite reflex effect on the behavior of the stomach.

The author employs a mixture of barium cream, commercially known as Ramul, made in three thicknesses: No. 1, thick, flavored, for esophageal cases; No. 2, medium thickness, flavored, for gastric cases; No. 3, thin, unflavored, for opaque enemata. These, being sterilized, keep for weeks, and therefore can be kept in stock. The barium itself is well held in suspension. The prescription is as follows:

No. 1. Thick flavored.		
Barium sulphate		10 oz.
Saccharin		2 gr.
Vanillin		5 gr.
Gum tragacanth		100 gr.
Distilled water	to	20 oz.
No. 2. Thin flavored.		
Barium sulphate		10 oz.
Saccharin		2 gr.
Vanillin		5 gr.
Gum tragacanth		60 gr.
Distilled water	to	20 oz.
No. 3. Thin unflavored.		
Barium sulphate		10 oz.
Gum tragacanth		60 gr.
Distilled water	to	20 oz.

The gum tragacanth and the barium sulphate should be mixed as powder, and the water added gradually. The whole is sterilized. It is better made up by a chemist, and a stock bottle can be kept for emergency. Being sterilized, it can be used for other purposes, such as injecting into sinuses or empyemas, using whichever thickness is most suitable for the case. This barium cream throws a very dense and homogeneous shadow, creeping readily into nooks and crevices, such as ulcer craters, appendices, etc.

No hard and fast rule is made as to amount of meal to be given in a given case; 3 to 4 oz. given at first, a further 6 oz. or more to be administered later as required.

Since pressure of work makes necessary the elimination of everything that does not prove to be of some diagnostic value, one of the first things investigated was whether the emptying time of the stomach gave any reliable information as to the state of the organ itself. The author can recall no case in which the emptying time of the stomach was of help in arriving at a diagnosis; the emptying time is influenced by so many unknown factors that he doubts its value. He claims that it is possible to obtain a difference of four hours in the emptying rate of the stomach in the same patient by starving or overfeeding him a day or two before the examination.

The author has also discarded the double meal method, as he has found that although in theory it should save time in actual practice, it may lead to serious diagnostic error. He therefore depends on the one meal, which he follows through into the intestines.

In order to carry out screen work expeditiously, every examination should develop a systematic routine. A systematic method should be employed in all fluoroscopic work. The presence of one lesion, a gastric ulcer, for instance, does not exclude the possibility of another lesion, a duodenal ulcer, for instance. The apparatus should be simplified. Protection should be hung around the apparatus rather than on the examiner. The protection should be ample.

Palpation is an important factor in this method. Some practice is necessary before the knack is acquired, and on it depends, to a great extent, the success of the method. It is necessary that the hands be trained to work in conjunction with each other, as each section of the stomach is examined in detail. Screen palpation should be done in both the horizontal and upright positions.

MacNeal, Ward, J., and Willis, George S. A skin cancer following exposure to radium. *J. Am. M. Assn.*, Feb. 17, 1923, lxxx, No. 7, p. 466.

The authors report in detail, with numerous photomicrographs, the case of a patient who worked with roentgen rays in his practice from 1905 to 1917; but not since then. Precautions for self-protection were carefully employed. From 1912 to June, 1920, he handled radium, without precautions for self-protection, in small amounts up to 1915, but in quite large amounts from 1915 to 1920 (from 200 to 365 mg. in individual tubes, taken between the right thumb and forefinger almost every day). Various changes, which may be ascribed to the exposure to radium, began to be observed late in 1918, and since early in 1920 the skin changes have required constant care. In September, 1922, a fissure on the ball of the right thumb manifested a peculiar and extremely painful alteration in character, and on excision in October, this lesion proved to be a squamous-cell carcinoma.

Lemere, H. B. Progressive osteomyelitis of the frontal bone. *J. Am. M. Assn.*, Mar. 3, 1923, lxxx, No. 9, p. 596.

The author discusses a very interesting case of diffuse osteomyelitis of the frontal bone, progressive in nature, ending in death, through infection of the frontal sinuses. A basal meningitis complicated the case. An excellent review of the literature on the subject to date then follows. The etiology of this disease is still mysterious and the mortality with or without extensive bone removal is exceedingly high. The author calls particular attention to the necessity of publishing the roentgenograms rather than a statement of the roentgen findings, in order to make it possible carefully to study the literature of the subject. Bacteriological reports should also be made, and where death results, careful necropsy findings. In 21 spontaneous cases gathered from the literature, there were 7 recoveries, while in 20 postoperative cases, there were no recoveries. In none of the literature is the importance of the roentgen ray observations on the osteomyelitis sufficiently emphasized. The author believes the treatment and operative work should be checked by roentgenograms taken at least once a week, and all bone showing any involvement should be carefully removed.

Renaud, Maurice. The administration of morphin to cancer cases. *Ass. franc. p. l'etude du cancer*, Mar. 19, 1923.

The author reviews the dangers and inconveniences of morphinization in the course of chronic affections and declares that in spite of the temporary and often illusory advantages, the administration of morphin brings to the cancer patient no real benefit. On the one hand, the painful paroxysms, for the relief of which morphin is the sovereign remedy, are relatively rare in the course of the majority of neoplasms; and the majority of symptoms which render the cancer patient's life miserable are not entirely ameliorated by stupefying medication. Cancerous morphin addicts are the ones who complain the most. On the other hand, morphin is not in any way capable of attenuating the mental distress of the patients who suspect or know they are hopelessly condemned. Far from giving mental relief, morphinization encourages and develops disquietude. The morphin addict never knows true rest. He is in the state of perpetual malaise. Morphinization which increases the torments and accentuates the mental distress of the cancer patient is therefore a dangerous last resort. The cancerous patient is a depressed neurasthenic who should be sustained, stimulated, and encouraged. Morphin is good for nothing until it becomes necessary to relieve his pain.

Schenck, W. E., and Goosmann, Charles. Melanotic sarcoma of eye. *Cincin. M. J.*, Mar., 1922, iii, 1.

The authors decry the reports of cured melanotic sarcoma of the eye after only two

or three years with no recurrence after surgical treatment. Such reports lead us into a false position of security. In order to insure cures, the authors query, "Should we use *x*-ray or radium first and then operation, or should we depend upon the *x*-ray or radium treatment entirely?" They conclude that operation should precede treatment with irradiation; that the irradiation should be administered only by an expert; and that under no circumstances should the use of an artificial eye be permitted.

BUXTON, ST. J. D. The association of the surgeon and radiologist in bone grafting. *Arch. Radiol. & Electroth.*, Mar., 1923, xxvii, 289.

The author believes the assistance given to the surgeon by roentgenography in the diagnosis of fractures and diseases of bone is one of the outstanding features of the progress of science. Assistance of the *x*-ray workers is valuable in experimental work, especially on man in watching the progress of each case of bone graft. In studying the indications for bone grafting in a long bone the roentgenogram shows the condition of the bone and thus assists in making a choice of bone grafting in each type of case. The *x*-ray help is especially useful in the avoidance of failure to obtain union after operation by bone grafting. This failure may be due to the following causes:

1. Waking up of latent infection, and so causing suppuration in the wound.
2. Poorly nourished covering of the graft owing to deep scar and skin cicatrices.
3. Defective technique in application of the graft.
4. Fracture of the graft.

The value of roentgenography in watching the progress cannot be exaggerated. In the case that does well it is by the roentgenogram as much as by the apparent strength of the limb that we can judge for what period a splint should be worn.

A series of very interesting roentgenograms is appended to the article.

BOST, T. C. Calcification of the ovary. *J. Am. M. Assn.*, Mar. 31, 1923, lxxx, 912.

Moschcowitz in 1916 compiled 5 cases. Dicken has since added another case. The author's case herewith reported in detail brings the total to 7. The specimen measured about 5 by 3 cm., occupying the site of the ovary. Normal Fallopian tube was attached to the upper surface. The general shape of the mass was that of a flattened egg. The surface was smooth, mottled yellow; but the tissue was of uniform bony hardness. Diagnosis—calcification of the ovary.

ROBERTS, C. S. LANE. Some considerations of the sacroiliac joint. *Lancet*, Apr. 21, 1923, cciv, 787.

This is a well-prepared consideration of the morphology, gross anatomy, movements and the more common pathological conditions of relaxation and strain of the sacroiliac joint. Although no reference is made to the employment of the *x*-rays in the diagnosis of this condition, the facts presented should be of interest in the interpretation of the *x*-ray findings in this situation.

HODGES, FRED M. Subdiaphragmatic abscess. *J. Am. M. Assn.*, Apr. 14, 1923, lxxx, 1055.

This very interesting article is well worthy of detailed study by every reader of this journal. Early diagnosis and treatment usually lead to cure, while late diagnosis often means serious complications or death. A history of a recent abdominal operation or infection, followed by an unexplained elevation of the pulse and temperature, should require a careful elimination of subdiaphragmatic abscess. The roentgen ray is a very important diagnostic aid, and will almost invariably give valuable and definite information. In any acute upper abdominal infection, an elevated and rigid diaphragm should lead to the suspicion of subdiaphragmatic abscess. A diaphragm normal in position, contour and motility usually eliminates the possibility of an abscess just beneath. Promiscuous needling is never indicated, since in this way the pleural cavity may be unnecessarily infected.

FRIEDLAENDER, BERNHARD. Diagnostic value of artificial pneumoperitoneum in sterility in women. *J. Mich. M. Soc.*, Apr., 1923 xxi, 195.

The author concludes that in sterility of the woman we should undertake a routine and thorough examination of the organs of reproduction in the male as well as in the female and try to find the cause of sterility. The patency or non-patency of the Fallopian tubes can only be determined, without a surgical exploration, by the inflation with gas by the transuterine route. Pneumoperitoneum is free from danger if we are careful in the selection of cases. Roentgenograms of the pneumoperitoneum are valuable aids in demonstrating the condition of the uterus, tubes and ovaries, the presence of omental and bowel adhesions, pregnancy, etc. The use of carbon dioxide instead of oxygen diminishes the discomforts following the inflation and the patient is able to leave the office in thirty minutes.